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THE CANALS OF MARS, OPTICALLY AND PSYCHOLOGICALLY CONSIDERED

A REPLY TO PROFESSOR NEWCOMB

By PERCIVAL LOWELL

When my revered master, the late Professor Benjamin Peirce, asked me at graduation from Harvard to return to the university to teach mathematics with a view to becoming eventually professor of the subject there, I refused because I preferred not to tie myself down—not because mathematics had not always appealed to me as the thing most worthy of thought in the world. Indeed, subsequent companionship with this, perhaps the most attractive of all the muses, has impressed me with two qualities about her of fundamental importance in any inquiry, both of which find exemplification in the article to which I am about to reply. The first of these is that mathematics is merely formalized logic and deals not with the matter concerned but with the manner of its manipulation. If you put peascods into a machine you cannot take out flour, however fine you grind. Or, to drop analogy, if you apply mathematics to physics or optics, you must first be sure your physical data are correct before you proceed to deduce results from them. The second fact about mathematics is that it is a quantitative, and not simply a qualitative, instrument. Now, in the cosmos, quantity—size, length, duration—is of the very essence of things and processes. A body with twice the diameter of another does not behave under similar conditions—stress, temperature, motion—with twice the action of the first. Or, to take another

kind of example, an invention may work beautifully on one scale to fail completely on another. It is not enough, therefore, to examine a matter qualitatively; to reason quantitatively is vital to reasoning aright.

Before proceeding to take up Professor Newcomb's article on "The Optical and Psychological Principles Involved in the Interpretation of the so-called Canals of *Mars*,"[1] which I am about to answer, it is a source of pleasure to note two things in it: first, that it shows how much the subject of recent Martian investigation has excited interest in scientists not themselves specialists in the subject; and secondly, how completely the general attitude of incredulity has altered for the better in the last ten years. In addition, it is pleasant to perceive that Professor Newcomb recognizes the importance of critical experiments on all points, even hinting at illusion, and appreciates the great care that has been taken at this observatory to examine and prevent any such possible mistakes.

To deal now with the points he has raised in the order in which he raises them: The first relates to the fact that any so-called achromatic objective causes of necessity rays of different colors to come to a focus at different focal distances and consequently that if any monochromatic image be focused on, it must perforce be surrounded by others out of focus and therefore distended in area. This, of course, is true, but Professor Newcomb then goes on to argue numerically from it without noticing the omission of two factors in the problem which entirely change the results at which he would arrive. The first of these is that the very fact that the out-of-focus images cover more area dilutes their effective character and *as the square* of their diameters. Secondly, the intensities of the various colors as light for the eye is not, as his computation tacitly assumes, equal at the start; but, on the contrary, form a curve sharply rising from invisibility in the ultra-violet to a sort of flat-topped cone in the yellow, to fall with like abruptness to disappearance in the infra-red. In other words, the eye itself discards to a great extent what is out of focus. Each ray has to be multiplied by $\frac{1}{(F_1 f)^2, F_2 f}$, where F is a function depending on the color-curve of the objective, and F_2 that of the sensitiveness

[1] *Astrophysical Journal*, **26**, 1–17, July 1907.

of the human eye to the focal length f of any ray—and then the several rays integrated for so much of each as falls within the distance s, before the least approximation can be made either to the amount of light as regards the eye within a circle of radius 0″.10, or to the blurring effect of the unfocused to the focused portion. So that Professor Newcomb's determination is not even an approximate solution of the problem.

The effect of these two factors is to produce a curve of intensities of the special colors for the eye quite other than he considers to be the case and not very unlike that of the intensity of the diffraction pattern for a circular aperture. Now every observer of double stars is aware that a striking feature of the spurious disc made of a point of light by a lens is the clear-cut character of its edge. Instead of fading off gradually the light ends abruptly, to begin again sharply in the first bright ring. Thus experience shows that the eye takes account only of certain intensities, refusing to perceive the rest. And this is explained by the introduction of the two factors omitted by Professor Newcomb.

Thus we see that Professor Newcomb's investigation is theoretically incorrect because of the omission of two factors vital to the result. Let us turn from this to the published experience of an expert optician. For it has escaped Professor Newcomb's notice that this subject has been not only theoretically but practically treated with much care already by a man who stands high in his profession, to wit, by the optical manager of Messers. Cooke & Sons, Mr. H. Dennis Taylor.[1] Although the whole paper is pertinent to the subject, the following brief quotations will suffice:

> Now it happened to occur to me that I would determine the limits of good focus, not only theoretically, by application of the usually accepted rule, but I would also confirm it by experiment. What, then, was my surprise when I found that, so far from there being any agreement between the tacitly accepted theory and actual fact, I actually had to push in the eye-piece or draw it out in order to expand the spurious disc into a penumbra of twice its size, by an amount equal to 0.03 inch on *each* side of focus, or *five* times as much as I had theoretically expected; while I could push the eye-piece in or out by 0.015, or nearly 0.02 inch, without sensibly increasing the size or destroying the character of the spurious

[1] *On the Adjustment and Testing of Telescopic Objectives*, T. Cooke & Sons, pp. 67–84; also "The Secondary Colour Aberrations of the Refracting Telescope—Relative to Vision."—*Monthly Notices of the Royal Astronomical Society*, **54**, 67–84, 1893.

disk. This astonished me, and led me to make a series of experiments with different eye-pieces, apertures, and focal lengths and varying relations between apertures and focal lengths. The results turned out to be independent of the focal lengths of the objective and the power of the eye-piece, provided the magnifying power was sufficient to show the star disk.[1]

And below:

But here we have a fact which renders possible what before seemed impossible, viz., the production of a tolerably definite focus out of the confusion of many foci; for since the bundle of rays is nearly cylindrical for a distance of nearly 0.02 [inch] on each side of focus (when $\frac{f}{a}=15$), evidently color aberrations to that amount on each side of focus will not give rise to loss of light owing to any part of it falling outside the limits of the star disc.[2]

Thus his tests are entirely opposite in conclusion from what is supposed to exist by Professor Newcomb.

Now let us see what takes place in practice at the telescope; for all our physical, in this case optical, knowledge is ultimately based on experience and experiment. As I said in the beginning of this reply, if we put into our equations wrong fundamental facts we shall only derive from them wrong conclusions. We will select, to start with, optical phenomena which have nothing to do with *Mars*, as being less open to criticism. In the clear and steady air of Flagstaff, the shadows of *Jupiter's* satellites upon the planet's disk stand out so sharply edged that they give the impression at first of being actual black spots in the focal plane of the eye-piece. This is in spite of possessing, of course, a true penumbra, apart from any telescopic effect. This clearly defined appearance has been noticed not by one only but by all the observers here. Secondly, ink lines have been ruled on paper of known widths and observed through the telescope at suitable distances to have them subtend angles analogous to planetary lines. They have uniformly been observed to be as dark and unfringed of shading as when seen close to by the naked eye. This is the consensus also of all the observers scanning them. Lastly, faint shadings are seen on some planets which are totally different in appearance from the "canals" of *Mars* as here seen. Thus on *Venus* such exist; and in the wisps between the belts of *Jupiter* (actually photographed here recently by Mr. Lampland) we have another variety which no practiced observer would liken to the Martian lines.

[1] Cooke, *op. cit.*, p. 74. [2] Cooke, *ibid.*, p. 77.

Thus, experience at the telescope, quite apart from anything to do with *Mars*, and experiment there as well, are definite in their pronouncement against Professor Newcomb's supposed optical effects.

Before leaving the optical part of his paper I may mention the causes of what he considers discrepancies in drawings between different observers. For none such are to be found here between observers working consecutively, though each at the time be ignorant of the other's delineations. Poor air is the first condition of seeming discrepancy and the cause of such drawings as show the lines broad and diffuse. It produces the same distortion that we mark over a hot stove, and in the case of fine detail spreads what would be dark and narrow into a wide grayish belt. This I know, not only from theory and inter-comparison of the drawings of others, but from having seen the effect produced and afterward subsiding at the telescope.

The second cause is seasonal change in the planet itself. The "canals" are not at all times equally conspicuous. Indeed, they undergo a regular annual metamorphosis, and what is more and very interesting and peculiar, they individually suffer what may be called hibernation for a greater or less length of time, up to one or more Martian years.

Turning now to the second portion of the paper—that dealing with psychological principles, we come to the second point I have spoken of as being vital, to wit, quantitative, as opposed to merely qualitative, investigation. A reader not conversant with the subject would suppose that the disk constructed by Professor Newcomb and observed at the given distances away represented in size and consequent suitability for depiction that of *Mars* as seen through the telescope when the scanning of this disk is most fruitful. In fact he so supposes it himself, as he tells the reader that "its breadth and distance were arranged so as to correspond to the apparent disk of *Mars* under the usual magnifying power." Calculation, however, will show this not only not to be the case but widely to differ from the fact. At this opposition the disk of *Mars* subtended 23″, and, of course when criticism of the linearity of its "canals" or of the fineness of its markings is in question, it is by the best not by the worst or even by the medium views of the planet obtainable that our knowledge of them is derived and therefore to be gauged. The lowest power with which observations here have been made of *Mars* at this opposi-

tion is one magnifying 391 diameters. With a power, then, of 391 and a disk of 23″, the apparent size of *Mars* is 149′.5 or 2°.5, five times the diameter and twenty-five times the area the moon presents to the naked eye. Now the disk used in Professor Newcomb's experiment was 38 cm in diameter and the distance at which it was used by the naked-eye observers was 30 meters and 100 feet. It subtended therefore but 44′, or less than a third of the diameter, and less than one-eleventh of the disk of *Mars*. In other words, *Mars* spreads an area to the observer's eye between eleven and twelve times that shown by the experimental disk. Interesting, therefore, as this experiment may be, it has no applicability to *Mars*. This any reader of the paper can easily prove to himself. In order for the disk as reproduced in the article to look as large as *Mars*, it must be viewed from a distance of 2.7 m (8.7 ft.). The observer will then find that not only is he under no liability of illusion to make of the discontinuous markings lines, but that he can see their shape and character with great nicety.[1]

The fact is that here quantitative treatment, or in other words, the scale on which the experiment is tried, is vital to any importance in the results we are to deduce from it. No one would dream of denying that there is a limit to detection of any detail whether telescopic or of everyday action. It is a truism. We cannot affirm that below that limit even a telegraph wire is continuous. In fact we know from the molecular theory of matter that it necessarily ultimately is not. In questioning, then, the apparent linearity of the "canals" of *Mars* it is necessary to make quantitative measures to realize where we stand. Now, such measures have been made at Flagstaff; and the experiments were made through the telescope. This is an important detail to be observed in all such investigations. When an astronomical fact is to be investigated experimentally, it is fundamental that the phenomena should be subjected to telescopic

[1] Even at the distance (30.3 ft.) corresponding for the reproduction to the erroneous distance at which the original was sketched, nearly four times as far off as it should have been, Mr. Slipher and the writer found with only a general knowledge of the reproduced disk no difficulty in making out its dotted character. The greater part of the supposed lines showed to the writer their composite character at once, thus failing to simulate the uniformity of the Martian canals; while in some instances he could see the actual breaks between the dots.

result. The only way to test the action of an unknown factor is to exclude all known factors of variation from the experiment, or, in other words, to vary the conditions only as regards the factor to be found. Self-evident as this is scientifically, it has not been put in practice generally by critics of the markings on *Mars*.

Now in this connection, to observe markings by transmitted light, as is suggested by Professor Newcomb, is to violate the above principle, well recognized as it is in all research. For the planet is lighted to us by reflected light and we must observe our experimental markings in the same manner if we would attain precision in our results. I may also say that in my paper on visibilities of fine lines, which Professor Newcomb quotes, not only was the ***minimum visibile*** determined, but the limits of visible inference, to wit, 0".59, in the case, which he missed in reading the paper. He will also be interested in knowing that the illusion to which he found himself subjected, that of seeing lines caused by shading on the paper, similarly presented itself to me many years ago in telescopic investigation of this very subject; but only when nearer the limit of contrast than is the case with the "canals" of *Mars*. Again the wire, which he supposes to have been black and from whose color he argues to his conclusions, was not black; so that the deductions in consequence fall to the ground. Furthermore, the results were practically the same when the experiments were repeated with dark blue[1] lines on a paper background, so that the sky was not in question. This emphasizes again the absolute necessity of quantitative measuring in the matter. For it is part of the profession of a trained observer to recognize just such points. And here I may correct an impression which Professor Newcomb, not being himself an observer of *Mars*, has received at second hand. He states that the background upon which the "canals" are seen is not uniform in the case of *Mars* and that therefore lines on paper are not a true criticism. This is an error due probably to his reading that the "seas" were a jumble of markings impossible to decipher. This jumble is the very canal and oasis system imperfectly seen, as I can state from having seen it resolved. It, therefore, cannot be used as an argument against its own detection after the fact—especially in the light regions where uniformity of tint is the rule. He was arguing

[1] *Lowell Observatory Bulletins*, Nos. 2 and 10.

from the observations of a sensitive and not an acute eye; a very pregnant source of mistake. For experience shows that an eye good for faint star and satellite work is constitutionally defective for planetary detail and vice versa; a fact dependent apparently upon the size of the retinal rods and cones.[1]

Lastly, I may remark parenthetically that Professor Newcomb's theory leads him with his supposed fringe for the canals (p. 15, bottom) to a real width much smaller than the visible one, so that he actually strengthens unwittingly the argument for their fineness. This brings us back to what is visible in a telescope.

To begin with, the "canals" of *Mars*, seen through the telescope with good definition at Flagstaff, are not diffuse streaks but narrow definite lines. Now, as Professor Newcomb with justice and good judgment remarks of these very observations: "what is seen by a single practiced observer under the most favorable conditions affords evidence which completely outweighs those of less-favored observers." Indeed it is evident that what is seen by a trained observer under good definition cannot be disputed by failure to see by others; *a fortiori* when all the observers at a station chosen for just this purpose concur, which is the case here. Now the observations at Flagstaff are perfectly definite on the point. In fact the Martian canals actually appear darker and more pronounced than do the writer's drawings of them when telescopically viewed to subtend the same angle.

Next we come to the experimental tests also made at Flagstaff through the telescope upon lines ruled on paper with ink, set up at a distance of 585 feet. The lines were of various character: straight and uniform; linked by having some parts thicker than others; broken, the breaks being of different dimensions. The lines were prepared by Mr. Williams at this observatory, and their configuration and special character were unknown to the observers.

The distance from the 4-inch Clark achromatic to the board upon which the paper was tacked was 178,000 mm. One-tenth of a millimeter on the paper subtended, therefore, 0″.116 to the naked eye, which with a power of 28 on the telescope became 3″.24 and with one of 37, 4″.29.

At opposition this year *Mars* was roughly 39,000,000 miles away.

[1] See Webb, Williams, Lowell, on the subject.

One mile at that distance seen with a power of 391, the one commonly used on the 24-inch at this opposition, subtended therefore 2″.06. Consequently 0.1 mm in the experiment viewed with a power of 28 = 1.57 miles on *Mars;* with a power of 37, it = 2.1 miles. Now, in the first place, the linked lines were instantly seen to be of irregular width and the number of irregularities counted correctly with a power of 37 by both Mr. Lampland and me when the difference between the thicker and thinner portions was only 0.15 mm. Now with the eye-piece this equaled three miles on *Mars;* the width of the links in the case being eight miles and five miles. Thus the difference between a canal of eight miles and one of five was discernible.

In the next place a break in the lines of as little as 0.75 mm was visible; while one of 0.6 mm was so at times, though at others the line showed continuous. Seventy-five hundreths of a milimeter is a quantity corresponding to twelve miles (19 km) on *Mars;* six-tenths of a millimeter to nine and one-half miles (15 km).

The narrowest line drawn, which was by no means at the limit of vision, was 0.12 mm wide and was easily visible with a power of 28. This equaled less than two miles (3 km) on *Mars.* Mr. Lampland's observations substantially agreed with mine in these particulars.

The telescopic verdict at Flagstaff, then, as to the detection of irregularities and breaks in seemingly regular lines is:

1. A difference in thickness which corresponded in angular dimension to three miles on *Mars* was perceptible telescopically;

2. A break, analogously, exceeding eighteen miles (29 km) on *Mars* was discernible;

3. A line of a width of less than two miles (3 km) on *Mars* was easily visible.

We are therefore confronted with the following alternatives: either the "canals" of *Mars* are as they seem, narrow straight lines; or they are syntheses of small markings which show the following surprising characteristics: the pieces never differ by more than three miles in width; secondly, are never farther apart than eighteen miles (29 km); thirdly, are arranged in a perfect row for twenty-five hundred miles (4000 km), more or less, and lastly, exhibit this remarkable connection not in one but in every instance, to the number of over four hundred, in which they occur. If we double these figures, even,

to allow for greater magnification in the telescope, the oddity is not substantially changed. Anyone acquainted or even unacquainted with the laws of probability will have no doubt in coming to a decision as to which alternative is the more probable.

LOWELL OBSERVATORY, FLAGSTAFF, ARIZ.
August 5, 1907

NOTE ON THE PRECEDING PAPER

By SIMON NEWCOMB

While it is a pleasure to appreciate the weight of Professor Lowell's argument, I cannot concede that either of the factors he mentions has been omitted by me in a way to change the character of my results. As to the second of the omissions, my results are based on light between λ 5614 and λ 5894, a region in which the light-intensity is nearly uniform, and not on the fainter red and violet light, the slight effect of which is rightly emphasized. As to the other point, Mr. Lowell seems to overlook the wide difference between the shading off of the dispersed light around the sharpest image of a luminous point, and the diffusion by aberration of the image of a black point on a bright background. In the last case, the image does not consist of a black central point rapidly shading off as the inverse square of the distance from the center, but of an ill-defined half-tone, shading off much less rapidly. This is true in a yet greater degree of the image of a dark line on a bright background, which is the case of the Martian canals.

Mr. Lowell's citations from Mr. H. Dennis Taylor and his own useful telescopic observations of artificial dark lines on a light background, afford excellent illustrations of the process of visual inference described in my paper (pp. 8, 9). To apply them it is only necessary to compute the actual breadth of the images of the lines on the observer's retina, and compare them with what the observer "sees" or thinks he sees.

One word to correct a possible misapprehension of the bearing of my argument. So far as it goes, the canals of *Mars* might be fine lines of inky blackness. It only seeks to show that there is an indefinite number of other features which an observer may train himself into interpreting as fine dark lines, and that the actualities on *Mars* may therefore differ widely from the observer's optical inferences.

I shall be glad if Mr. Lowell will either distinctly accept, or revise if necessary, my computation of the area of the image of the entire canal system on the retina of the eye, and investigate the optical effects arising from it.

REPLY TO PROFESSOR NEWCOMB'S NOTE

By PERCIVAL LOWELL

Professor Newcomb's note has been sent me with the question as to whether I would reply to it. It need only be said:

First, that it is precisely to light between $\lambda = 5614$ and $\lambda = 5894$ that Mr. Taylor's experiments referred.

Second, that the general mathematical treatment is the same for a bright line on a dark area, or a dark line on a bright one, as only the light-disturbance can be integrated in either case, the former being slightly widened, the latter slightly narrowed in consequence; and I particularly showed, i. e., by a star disk and by the shadows of *Jupiter's* satellites, that in both cases no haziness was produced perceptible to the eye.

Third, instead of its being true that Mr. Taylor's and our experiments afford excellent illustrations of the process of visual inference described by Professor Newcomb, the exact reverse is the case, as shown in my paper and as Professor Newcomb will find when he shall subject his a priori supposition to actual experimental tests with instruments.

Professor Newcomb's request for a revision of his computation as to the area of the image of the entire canal system on the retina of the eye I am very glad to comply with, though it only brings out more strikingly the linearity of the canals. From their zonal numbers and breadth, which is too small to disclose any sensible width and can be got only by comparison of intensity with the micrometer thread and by other experiments, giving fifteen miles as the maximum width of the average canal, their area comes out approximately $\frac{1}{170}$ of the surface of the planet. As to the retinal area, it is probable that when a single cone is struck it responds *in toto* and indivisibly to the stimulus, gauging size solely by intensity. With a disk of 23″ and a power of 393, the *minimum divisibile* on *Mars* is 28 miles (45 km), i. e., a single cone corresponds to this space. Therefore, as each canal shows no perceptible breadth, it wakes a single line of cones only and therefore cannot possibly show perceptible shading at its sides, while the retinal area, if such we may call it, is twice the above value. If the cone does not respond *in toto* the area diminishes to the above as its limit.

THE WEAKENED AND OBLITERATED LINES IN THE SUN-SPOT SPECTRUM

By G. NAGARAJA

Of the diverse features in the spectra of sun-spots the widened lines alone have received the greatest attention from observers. They are no doubt the most conspicuous. Several lists of them have been published, but so far some other peculiarities of the spot spectrum are only just beginning to receive attention. Especially is this the case with the weakened and obliterated lines. They have certainly been long recognized as characteristic of spots, but their number and character have yet to be properly estimated. Dr. W. M. Mitchell in an exhaustive catalogue of spot-affected lines between *a* and F has recorded for a total of 680 such lines about 50 as enfeebled.[1] In a later list and in connection with an allied phenomenon in spots, "the reversed lines," he has increased their number.[2] Messrs. Hale and Adams in their photographic observations of the spectra of spots have included but 26 weakened lines in a catalogue of about 345 which are affected in spots.[3] In another paper[4] dealing with the temperature of spots they have taken into account only 32 enfeebled lines for the whole region from λ 4060 to λ 5860. Professor Fowler[5] has considered about 30 weakened lines which he has found to belong to the high levels of the chromosphere. Some visual observations of mine on several large spots made me suspect that the weakened lines in spots were far more numerous than has been previously recorded. I have recently been enabled through the kindness of Mr. Evershed to obtain spectrum photographs of the large spots of May and June last. He found a concave parabolic grating belonging to Professor Michie Smith to be very good and mounted it for me in the Rowland spectrograph instrument of this observatory. This grating has a ruled surface of 1.8 inches (4.57 cm) with 15 028 lines to the inch (2.54 cm) and its focal length for parallel rays is 10 feet (3.05 meters), the radius of curvature being 20 feet (6.10 meters). By the use of a collimating lens the plates are actually exposed at a distance of about 12 feet (3.66 meters)

[1] *Astrophysical Journal*, **22**, 4, 1905.

[2] *Ibid.*, **24**, 78, 1906.

[3] *Ibid.*, **23**, 11, 1906.

[4] *Ibid.*, **24**, 185, 1906.

[5] *Monthly Notices*, **66**, 361, 1906.

from the grating. Astigmatism is avoided by placing the camera tube normal to the grating and using approximately parallel light. A solar image of about 4 inches (10.16 cm) diameter is formed on the slit by a Grubb lens of 6 inches (15.24 cm) aperture and 40 feet (12.20 meters) focus fed by a siderostat. A sliding shutter with V-shaped aperture is arranged on the slit-plate, which enables the length of the slit to be varied between wide limits, thus allowing different lengths of exposure to be made for spot and sun. The definition of the grating is fine and the resolution is very good in the third order, which on one side is particularly bright.

Several excellent photographs were obtained of the region of spectrum between D and F. Rather long exposures were needed, notwithstanding the brightness of the grating, on account of the small angular aperture of the 40-foot lens; usually between 3 and 4 minutes were required for a spot, and 30 seconds for the adjacent photosphere,[1] this ratio giving approximately equal densities to the two spectra under the atmospheric conditions prevailing here. The linear dispersion is about 1.45 tenth-meters per millimeter and the definition is good enough to show the close doubles b_3 and 5316.8 distinctly resolved on the negatives.

An examination of the plates indicates quite clearly that the previous estimates of the number of weakened lines in sun-spots have been too low. I have carefully gone over the portion D to F on the photographs and have catalogued (leaving all doubtful cases) clear instances of 167 lines which are either thinned, weakened, or obliterated in spots. That is about half as many as the widened lines in the same region. As to the general character of these enfeebled lines, they are all of intensities in the sun ranging from 5 to 000 on Rowland's scale. The enfeeblement is generally through one or two intensities on the same scale. The greatest has been through 4, observed in the case of a few lines belonging to iron. I have included in the table at the end only those that varied through one or more units. Half or intermediate intensities might have been used and would have added more to the list, but I was afraid it would involve doubtful cases. One chief

[1] A small direct-vision prism is generally used in front of the slit to separate the different orders, and this, or the use of absorbing solutions, further reduces the intensity of the spectrum.

characteristic of the enfeeblement is that it is solely a feature of the umbrae of spots. The weakened lines differ in this respect from the reinforced lines, which, except in the case of spot-bands, generally encroach into the penumbra. We could also easily recognize in the photographs certain types among the enfeebled lines. Some are merely thinned, others are weakened and appear a few shades less dark in the spot than in the sun. Among the latter several seem diffusedly to extend to either red or violet side and in rare cases on both sides. One class of lines, generally of small intensity in the sun, are wholly obliterated in the spots.

The first question with regard to these lines will be as to what extent they are characteristic of spots, whether they are a permanent feature or occur only in single spots. There is, however, nothing to warrant the latter view except perhaps the meager and scattered character of previous observations. It may be stated in this connection that visual observations of the weakened lines are by no means easy. Very fine weather and spots with large umbrae seem to be essential. They fail to catch the eye as readily as the widened lines. That is probably one explanation why the observations are so few. It may be plausibly suggested, however, that this phenomenon may be characteristic of some active spots only. But I have observed them in some quiet ones which showed no sort of disturbance as is usually indicated by the behavior of the hydrogen lines. The spots of May and June from the spectrum photographs of which the accompanying catalogue has been prepared did not appear to belong to the class of active spots. At least at the time when the plates were exposed there was no disturbance going on.

But if it be asked whether the same lines are affected in all spots or in the same manner it is certainly too early to attempt a definite answer to the question. Dr. Mitchell has expressed the opinion that they vary and that he has found more weakened lines in 1906 than in 1905.[1] My own impression, however, is that there is not much variation. I have compared the different observations for the region D to F. Of the 26 lines contained in Hale and Adams' catalogue, 22 are in my list. It was quite a surprise to me that even the estimates of the degree of enfeeblement were either the same or very close in

[1] *Astrophysical Journal*, 24, 78, 1906.

TABLE I

TABLE OF WEAKENED AND OBLITERATED LINES IN SUN-SPOT SPECTRUM PHOTOGRAPHED AT KODAIKANAL IN MAY AND JUNE 1907
SOLAR LATITUDE OF SPOT, 12 AND 13 SOUTH

Wave-Length	Origin	Intensity in Sun	Intensity in Spot	Remarks
4874.196	*Ti*	o	oo	p-*Ti*(Lockyer) almost obliterated in Hale's map
4874.926	*Ni*	o	oo	
4875.215	—	o	oo	
4876.586	*Cr*	1	o	p-*Cr* (Lockyer); Mitchell gives maximum weakening
4893.997	—	oo	—	Obliterated; see Note 1
4894.743	—	oo	—	Obliterated
4900.301	*Y*	2	1	p-*Y* (Lockyer)
4912.666	*Cr*	ooo	—	p-*Cr* (Lockyer). Obliterated
4914.150	—	2	1	Weakens and also seems to thin on the red side
4916.426	—	ooo	—	Obliterated
4918.190	*Fe*	1	o	Weakens and also thins on violet side
4924.107	*Fe*	5	3	p-*Fe* (Lockyer). Chromospheric line
4925.450	*Fe*	oo	ooo	Mitchell gives a line at λ 4925.75, probably this
4936.015	*Ni*	2	1	
4937.245	—	oo	—	Obliterated
4937.524	*Ni?*	3	2	Thinned
4945.622	*Ni*	1	oo	
4945.814	*Fe*	1	o	
4946.215	*Ni*	o	oo	Almost obliterated in Hale's map
4947.778	—	oo	ooo	
4965.351	*Ni*	o	—	Obliterated
4974.431	—	ooo	—	Obliterated; a broad dark spot-band falls over the place
4984.297	*Ni*	2	1	
4985.432	*Fe*	3	2	
4986.403	*Fe*	1	o	
4987.088	—	oo	—	Obliterated
4995.586	—	oo	—	Obliterated; a broad spot-band falls over the lines and extends to the violet side
.835	—	oo		
4996.558	—	ooo	—	Obliterated
4997.024	*Ni*	1	o	
4998.408	*Ni*	1	o	Thinned; Mitchell gives maximum intensity
4999.207	*Fe*	o	oo	Mitchell gives a line at λ 4999.69, probably this
5008.825	—	ooo	—	Obliterated; spot-band falls over the place. Only weakened in Hale's map
5010.396	—	oo	—	Obliterated
5013.871	—	o	oo	
5014.100	—	oo	—	Obliterated
5022.414	*Fe*	3	2	Weakened only slightly in Hale's map; see Note 2
5023.372	*Fe*	o	oo	Hale's map shows a spot-band over the place which is not seen in my photographs
5027.937	*Fe*	1	oo	Mitchell gives this line
5048.242	*Ni*	o	ooo	
.409	—	oo		
5052.338	—	o	—	Obliterated
5057.665	*Fe,Ni*	o	oo	A bright band falls over the line
5072.479	*Ti*	o	—	p-*Ti* (Lockyer). Obliterated
5082.526	*Ni*	2	o	Hale and Mitchell give in their list, but is not clearly shown in Hale's map

TABLE I—*Continued*

Wave-Length	Origin	Intensity in Sun	Intensity in Spot	Remarks
5084.279	*Ni*	3	2	A bright line falls just to the red side and thins the line on that side
5086.422	—	oo	—	Obliterated; a bright band falls over the line
5087.601	*Y?*	1	oo	Chromospheric line
5089.134	*Ni*	0	oo	
5094.594	*Ni*	1	0	
5103.142	*Ni*	1	0	Thinned
5115.566	*Ni*	2	1	A bright line falls just to the red side
5115.961	*Fe*	0	oo	
5118.112	*Mn*	oo	ooo	Thinned; bright bands fall on both sides of the line
5119.292	—	oo	—	Obliterated
5121.732	*Ni*	0	0	Hale and Mitchell give the line
.825	*Fe*	2		
5129.805	*Fe*	1	—	Obliterated; a broad dark band falls over the region. See Note 3
5132.843	—	oo	—	Obliterated; Hale gives the line
5140.992	—	oo	—	Obliterated
5147.273	—	ooo	—	Obliterated
5154.579	—	ooo	—	Obliterated; see Note 4
5158.152	—	oo	ooo	
5159.231	*Fe*	2	1	Hale and Mitchell give the line; see Note 5
5164.724	*Fe?*	1	0	Hale and Mitchell give the line
5165.080	*C*, —	ooo	—	Thins on the red side
5170.937	*Fe*	0	—	There is a close widened line on the red side
5176.737	*Ni*	1	ooo	A bright band on violet side encroaches into the line and thins it
5178.970	—	oo	—	
5186.073	*Ti*	2	1	p-*Ti* (Lockyer). Weakened and diffusedly extending to violet side
5188.079	*Fe*	1	0	Weakened and diffusedly extending to red side
5197.332	*Ni,Mn*	oo	ooo	
5197.743	—	2	ooo	p-*Fe* (Fowler). Chromospheric line
5198.108	—	0	oo	
5211.700	*Fe*	oo	ooo	Thinned
5213.515	—	ooo	—	Obliterated; Rowland's intensity for the line is too small
5215.737	—	ooo	—	Obliterated
5218.085	*Fe*	0	1	
.369	*Fe*	1		
5220.358	*Ni*	0	oo	
5226.707	*Ti*	2	1	p-*Ti*. Thinned; chromospheric line
5234.791	—	2	0	p-*Fe* (Fowler). Chromospheric line
5236.373	—	0	oo	
5237.497	*Cr*	1	oo	p-*Cr*. Weakened and diffusedly extending both ways
5239.992	—	1	oo	Weakened and diffusedly extending to red side
5257.100	*Sr?*	oo	ooo	
5264.976	—	0	oo	Hale gives the line
5271.464	—	oo	—	Obliterated
5275.148	—	0	—	Obliterated
5280.239	*Cr*	oo	—	p-*Cr*. Obliterated
5284.281	*Ti*	1	oo	Mitchell gives the line; thinned and weakened

TABLE 1—*Continued*

Wave-Length	Origin	Intensity in Sun	Intensity in Spot	Remarks
5284.787	—	oo	—	Obliterated
5292.762	*Fe*	o	oo	Weakened and diffusedly extending to violet side
5293.211	Awv?	oo	—	Obliterated; Mitchell gives this as a chromospheric line
5294.134	*Fe*	o	oo?	
5294.726	—	oo	ooo	Thinned
5306.040	*Cr*?	o	oo	5306.3 p-*Cr* (Lockyer)
5316.790	*Fe*	4	3	p-*Fe*. Hale and Fowler give the line; chromospheric line
5317.724	—	oo	—	Obliterated
5325.738	—	2	1	Fowler gives the line; chromospheric line.
5335.050	*Cr*	1	o	p-*Cr* (Hale). 5335.5 p-*Cr* (Lockyer)
5336.974	*Ti*	4	3	p-*Ti* (Lockyer)
5337.910	—	o	oo	Hale gives the line
5342.890	*Co*	1	o	
5359.389	*Co*	oo	—	Obliterated
5363.058	—	3	2	p-*Fe* (Fowler). Chromospheric line
5377.028	*Fe*	o	oo	Thinned
5381.221	*Ti*	2	1	p-*Ti* (Lockyer). Rowland gives this as belonging to *Fe*
5409.339	*Fe*	2	1	The line is thinned on red side but extends far into the violet side in spot
5411.428	*Ni*	1	o	Thinned
5414.279	—	o	—	Obliterated
5425.464	—	1	o	Hale and Mitchell give the line
5478.668	*Cr*	o	ooo	p-*Cr*
5494.063	—	o	—	Obliterated
5502.297	—	o	oo	
5503.286	*Fe*	1	o	Weakened and thinned
5508.840	*Cr*	o	—	p-*Cr*. Obliterated
5510.229	*Ni*	1	o	
5519.797	*Fe*	o	oo	Thinned
5527.033	*Sc*	3	2	p-*Sc* (Fowler)
5532.202	—	oo	—	Obliterated
5532.968	—	1	o	
5535.061	*Fe*	2	1	
5539.507	*Fe*	o	—	Obliterated
5560.434	*Fe*	2	1	
5561.464	—	oo	—	Obliterated; Hale's map does not show it
5620.715	*Fe*	oo	ooo	
5625.541	*Ni*	o	ooo	Thinned
5625.904	—	oo	—	
5636.925	*Fe*	o	ooo	
5637.339	*Ni*	1	o	
5637.632	*Fe*	1	o	
5640.538	—	o	—	
5641.206	—	1	o	
5645.830	*Si*	1	ooo	Hale and Mitchell give the line
5649.898	*Fe*,*Ni*	o	oo	
5650.209	*Fe*	1	oo	
5650.911	*Fe*	1	o	
5651.691	*Fe*	o	ooo	
5659.817	*Fe*	o	ooo	

TABLE I—*Continued*

Wave-Length	Origin	Intensity in Sun	Intensity in Spot	Remarks
5665.775	*Si*	1	oo	
5666.899	—	o	oo	
5669.258	—	1	o	Hale and Mitchell give the line
5669.962	*Ni*	o	—	
70.163	—	o		
5682.427	—	2	o	
5684.710	*Si*	3	o	Hale and Mitchell give the line
5686.757	*Fe*	3	2	
5690.646	*Si*	3	2	Hale and Mitchell give the line
5701.323	*Si*	1	ooo	Hale and Mitchell give the line
5704.960	A	o	oo	
5708.622	*Si*	3	1	Hale and Mitchell give the line; diffusedly extends to red side
5714.380	*Fe*	o	—	Obliterated
5731.984	*Fe*	4	3	Mitchell gives the line; not shown in Hale's map
5752.254	*Fe*	4	3	Weakened and diffusedly extending both ways
5753.860	*Cr*	1	oo	
5757.037	*Fe*	2	1	
5772.364	*Si*	3	1	Hale gives the line; diffusedly extending to red
5784.879	*Fe*	1	—	Obliterated
5785.498	*Fe*	3	1	
5793.292	—	3	2	Not shown in Hale's map
5798.077	—	3	1	Hale and Mitchell give the line; but not shown in Hale's map
5804.681	*Fe*	o	—	Obliterated; Mitchell gives the line; not shown in Hale's map
5831.821	*Ni*	1	o	
5835.645	—	oo	ooo	
5855.300	*Fe*	1	o	
5856.312	*Fe*	2	1	

[1] A faintly dark shading is seen just to the violet side of where this line ought to be in the spot.

[2] Hale and Adams give the line as decreased in weak arc.

[3] There is a p-*Ti* line close to the line at λ 5129.32.

[4] A bright line appears in the spot in place of the Fraunhofer line over a dark band that falls over the region. There is p-*Ti* close at λ 5154.24.

[5] A diffused dark band extends to red side from the line.

all the 22 cases. As to the remaining 4 lines which I had not included, I found they had all been given by Hale and Adams only a weakening of half an intensity on the Rowland scale. They had therefore been excluded from my table. Probably this close agreement between different observers could have been possible only by the photographic method in the study of the sun-spot spectrum. Fowler's method of estimating intensities should also be responsible for some of this accuracy. There is, however, less agreement between Mitchell's observations and mine, and I have noted in the table all those found in the former's

TABLE II (SUMMARY OF TABLE I)

Elements	Number of Weakened Lines in Spots	Number of Widened Lines in Spots	Enhanced Lines Weakened in Spots	Enhanced Lines Not Weakened in Spots	Remarks
Unknown....	59	94	—	—	
Fe.....	48	50	5	2	
Ni.....	26	7	—	—	
Si.....	7	—	—	—	
Ti.....	7	48	6	1	
Cr.....	9	42	8	—	(1)
Co.....	2	5	—	—	
Y.....	2	—	1	—	
Sc.....	1	—	1	—	
Mn.....	1	13	—	—	

Rowland's identifications *Ni, Mn, Sr?, C*, —, *A*, *A*(*wv*) have each one weakened line in the region.

[1] There is one *Cr* line at λ 5753.860 about which it is not known whether it is enhanced or not, as Lockyer's tables have not been extended to that wave-length.

TABLE III (CHROMOSPHERIC LINES)

Wave-Length	Origin	Observers[2]	Level of Chromosphere	Weakened or not	Remarks
4861.527 (F)[1]	*H*		High level		
4883.869	*Yt* (earth)	N.	Low level	No	
4900.301	*Y?*	Y. N.	Low level	Yes	p-*Y* (Lockyer)
4921.963	*La-Ti*	Y. N.	High level	No	
4924.107	*Fe*	Y. N.	High level	Yes	p-*Fe* (Lockyer)
4934.214	*Ba*	Y. N.	High level	No	
.277					
4993.699	—	N.	Low level	No	
.864	*Fe*				
5015.9	*He*	Y. N.	High level	No	
5018.629	*Fe*	Y. N.	High level	No	p-*Fe* (Lockyer)
5087.601	*Y?*	N.	Low level	Yes	
5167.497	*Mg*	Y. N.	Low level	No	
.678 b_4	*Fe*				
5169.069	*Fe*	Y. N.	High level	No	p-*Fe* (Lockyer)
.220 b_3					
5172.856 b_2	*Mg*	Y. N.	High level	No	
5183.791 b_1	*Mg*	Y. N.	High level	No	
5186.073	*Ti*	F.	Low level	Yes	p-*Ti* (Lockyer)
5188.863	*Ti*	Y. N.	Low level	No	p-*Ti* (Lockyer)
5197.743	*Fe*	Y. N. F.	High level	Yes	p-*Fe* (Fowler)
5200.355	*Cr*	N.	Low level	No	
.590	*Va*				
5204.1	—	N.	Low level	No	
5205.897	*Y*	N.	Low level	No	
06.265	*Cr-Ti*				
5208.596	*Cr*	N.	Low level	No	
.776	*Fe*				
5226.707	*Ti*	Y. N. F.	Low level	Yes	p-*Ti* (Lockyer)

TABLE III—*Continued*

Wave Lengths	Origin	Observers[2]	Level of Chromosphere	Weakened or not	Remarks
5234.791	*Fe*	Y. N. F.	High level	Yes	p-*Fe* (Fowler)
5237.497	*Cr*	F.	High level	Yes	p-*Cr* (Lockyer)
5264.976	—	F.	High level	Yes	
5269.723 E_2	*Fe*	Y. N.	Low level	No	
5276.169	*Fe?*	Y. N.	High level	No	
5284.281	*Ti*	Y. F. N.	High level	Yes	
5316.790	*Fe*	Y. N. F	High level	Yes	p-*Fe* (Lockyer)
5325.738	—	N. F.	High level	Yes	
5328.696	*Fe*	N.	Low level	No	
.747					
5336.974	*Ti*	F.	High level	Yes	p-*Ti* (Lockyer)
5363.058	*Fe*	Y. N. F.	High level	Yes	p-*Fe* (Fowler)
5371.656	*Cr?*	N.	Low level	No	
.734	*Fe*				
5381.221	*Ti*		High level	Yes	p-*Ti* (Lockyer)
5397.344	*Fe*	N.	Low level	No	
5405.989	*Fe*	N.	Low level	No	
5425.464	—	H. M. F. N.	High level	Yes	
5429.991	*Fe*	N.	Low level	No	
5434.740	*Fe*	N.	Low level	No	
5447.130	*Fe*	N.	Low level	No	
5455.671	*Fe?*	N.	Low level	No	
.834	*Fe*				
5527.033	*Sc*	N. F.	High level	Yes	p-*Sc* (Fowler)
5535.061	*Fe*	Y. N. F.	High level	Yes	

[1] Wings of $H\beta$ obliterated in spot which therefore appear narrower than on photosphere. $H\delta$ on another plate taken about the same time is very much weakened in spot.

[2] Y refers to Young, F to Fowler, M to Mitchell, H to Hale, and N to the present writer.

TABLE IV (SUMMARY OF TABLE III)

CHROMOSPHERIC LINES—44

WEAKENED IN SPOTS 18				NOT WEAKENED IN SPOTS 26			
High Level 14		Low Level 4		High Level 9		Low Level 17	
Enhanced 9	Not Enhanced 2	Enhanced 3	Not Enhanced 1	Enhanced 2	Not Enhanced 7	Enhanced 1	Not Enhanced 16
Fe−5	*Fe*−1	*Ti*−2	*Y*−1	*Fe*−2	*Mg*−2	*Ti*−1	*Fe*−12
Ti−2	*Ti*−1	*Y*−1			*La, Ti* 1		*Cr*−3
Cr−1					*Fe?* 1		*Mg*−1
Sc−1					*Ba*−1		
					H−1		
Unknown 3					*He*−1		

catalogue. Nearly all the lines given by Fowler are in my list and are also noted. But what bears most on the point is that almost

all the lines in my catalogue are distinctly enfeebled in the reproductions of the Mount Wilson photographs of spot spectra which we recently received from Professor Hale. Of the 167 lines in my list only 7 are not shown in the reproductions. Of these 4 are of very low intensity in the sun and are completely obliterated in my photographs. It would thus appear that Hale and Adams in the preliminary study of their photographs did not particularly look for this phenomenon but simply recorded those that thrust themselves on their notice when they were examining the plates for the widened lines. We may then as a result of this close agreement conclude that between the time of the Mount Wilson photographs, which were taken probably some time during the latter part of 1905, and the middle of the present year, there has not been any noticeable variation of the weakened lines in the spectra of sun-spots.

I shall next consider some points of interest disclosed in the catalogue. In the summary (Table II) I give a list of the elements concerned, with the number of weakened lines in each case. And as it may be useful to compare the behavior of the same elements in the production of the widened lines, I have given the latter as well. These are extracted from the tables of Hale and Adams[1] and relate to very nearly the same reigon as I have dealt with. In the light of the connection that has been recently traced between the enfeebled lines in spots and the enhanced lines of some of the elements I have, along with noting them in the larger list, summarized them also, in Table II.

Comparing the weakened with the strengthened lines in spots we find that a large proportion in both cases are of unknown origin. Then comes iron, contributing nearly an equal number to either phenomenon. There are about 250 other iron lines in the same region which are probably unaffected in spots. We can only gather that as between the two cases iron does not seem to have any particular partiality. But not so some of the other elements. Most of the titanium and chromium lines are widened, while the nickel and silicon lines exhibit a similar partiality for weakening. The last, it should be noted, has all its lines in the region enfeebled.

With regard to the enhanced lines that are represented in the list, I am indebted to Mr. Evershed for the identification of most of them.

[1] *Astrophysical Journal*, 23, 28, 1906.

Reference has already been made to the 32 weakened lines which Hale has considered in his paper on the temperature of spots and of which he has found 29 to be spark lines. There is, however, nothing like this proportion disclosed in my table and the enhanced lines form by no means a large fraction of the total number. But it is to be remarked that Hale has included in his inquiry the more refrangible part of the spectrum, which is especially rich in enhanced lines, and also that complete tables are not available for the other regions. Still, within the portion I have considered, there are about 40 spark lines belonging to iron, titanium, and chromium which are found in Lockyer's tables, and 4 more which have been recently noted by Fowler to be enhanced. Of these 19 are found in the catalogue, and 10 are too little affected to be included in it, but still they appear to be slightly weakened.[1] In the case of the rest, most of them are only of small intensity in the spark and a few are too faint to be seen either in the sun or in the spot. There are, however, some 5 instances of enhanced lines of tolerable intensity in the spark not being affected at all in the spot. They are λ 5169.07 and λ 5169.22 belonging to iron, λ 5188.87 to titanium, and λ 5502.9 and λ 5621.7 to chromium. Thus, though it cannot be said that most of the weakened lines in spots are spark lines, we see, however, that a great majority of the latter are weakened in spots. It is also to be noticed that almost all the titanium and chromium lines weakened in spots are spark lines of those elements. The only exception is that of λ 5284.281, which Rowland has identified as belonging to titanium, but which is not found in Lockyer's table of enhanced lines. In the case of iron, while most of the spark lines in the region dealt with are weakened, yet a large majority of the weakened lines of this element are not spark lines, or have not as yet been identified as such. We have already seen that iron was concerned almost equally with producing both the widened and weakened lines, while titanium and chromium contribute mostly to the widened lines. It is then significant that when some lines of the two last elements suffer weakening in spots they should be almost all enhanced lines. Messrs. Hale, Adams, and Gale have from the laboratory experiments found that the spark lines of iron,

[1] Two more enhanced lines are in the list, one of which has been assigned by Fowler to "proto-scandium" and the other by Lockyer to "proto-yttrium."

titanium, chromium, and vanadium, when passing to a weak arc, are either weakened or obliterated, while the ordinary arc lines are all strengthened.[1] This would lead to the conclusion that the conditions prevailing in spots are analogous to the weak arc, and the Mount Wilson observers have so inferred. It might certainly account for the enhanced lines being weakened in spots. But this view alone cannot explain the presence of so many other weakened lines in spots which have not yet been identified as spark lines.

It may also be interesting to compare the chromospheric lines with those weakened in spots, and Table III has been prepared for that purpose. Only the lines found in the chromosphere between F and D are considered. Most of them have been observed by me and their character as high- or low-level lines determined. To make the table complete as far as possible I have included five other lines from Fowler's list.[2] A summary has also been added (Table IV) from which we gather that only a fraction of the chromospheric lines are weakened in spots. It is brought out further that a good many of the weakened lines belong to the higher levels of the chromosphere; but at the same time the contrary statement cannot be made. This would imply that the cause of weakening is not to be traced to the mere circumstance of these lines being present in the upper chromosphere. An examination of Table IV further discloses that a large majority of the weakened lines in it are also enhanced lines. Leaving out the 3 unknown lines, we find 12 out of the 15 to be spark lines. It has already been noticed that the latter tended generally to weaken in spots. The enfeebling then in the present instance of most of the chromospheric lines that are also weakened in spots may be accounted for solely on the ground of their being enhanced lines at the same time. The predominance of the high-level lines of the chromosphere among the weakened may also be explained by the larger number of the enhanced lines being found in those levels.

In bringing this paper to a close I wish to express my thanks to Mr. Evershed for the valuable help he has given me in the course of its preparation.

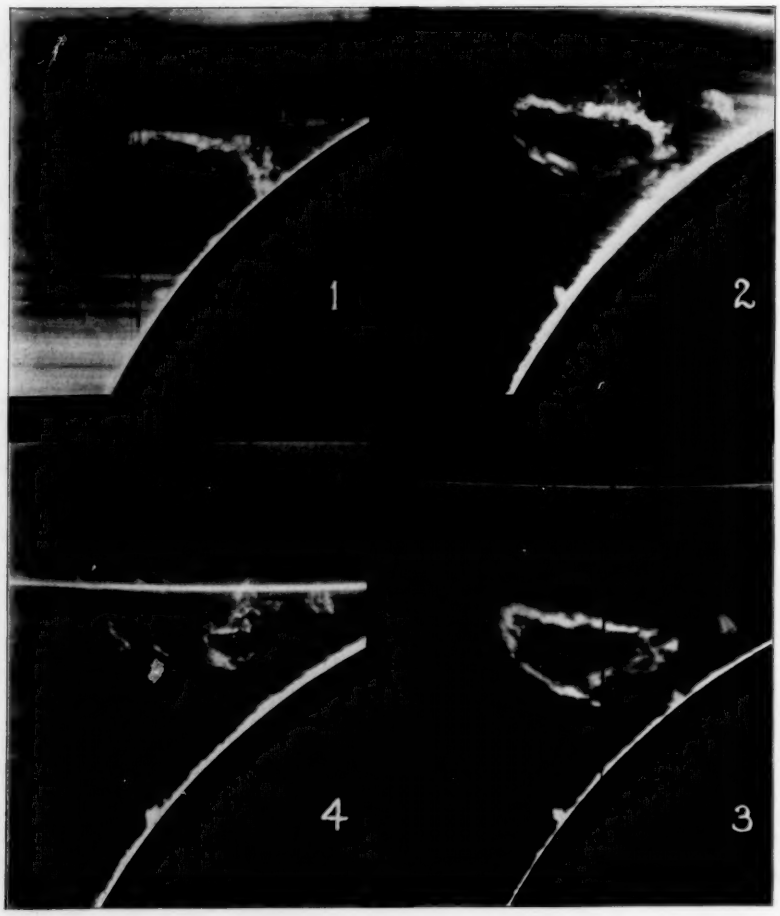
SOLAR PHYSICS OBSERVATORY
Kodaikanal, South India

[1] *Astrophysical Journal*, **24**, 185, 1906. [2] *Monthly Notices*, **66**, 364, 1906.

PLATE X

S

E



W

ERUPTIVE PROMINENCE, MAY 21, 1907
Photographed with the Rumford Spectroheliograph.

A LARGE ERUPTIVE PROMINENCE

By PHILIP FOX

The daily programme of observations with the Rumford spectroheliograph includes a photograph of the prominences around the entire disk of the sun, using the H line. Such a plate taken on May 21, 1907, upon development showed a prominence of unusual size in the southeast quadrant. I returned to the dome immediately to obtain other photographs. The sky was completely overcast with light cirrus clouds, so that the exposures were made under very unfavorable, almost prohibitive, conditions. However, I obtained a total of thirteen photographs the measurements of which give the following results. The position angle was roughly 138°.

Plate Number	No.	G. M. T.	Height	
Σ_s 2280.....	1	4^h 02^m	228".6	167,800^{km}
2281.....	2	4 52	259.3	190,300
2282.....	3	5 01	280.5	205,800
2283.....	4	5 35	391.6	287,400
	5	5 37	400.0	293,600
2284.....	6	5 43	431.8	316,900
	7	5 44	370.4	271,900
	8	5 44.5	370.4	271,900
2285.....	9	5 52	418.0	306,800
	10	5 53	415.9	305,300
	11	5 55	423.3	310,700
2286.....	12	5 57	410.6	301,400
	13	5 59	412.7	303,000

After exposure No. 6, the clouds so increased in density that the plates are of little worth. The measurements are consequently very unreliable, the fainter, outlying portions being obscured in some instances. They show, however, that the prominence was rapidly disintegrating. Cloudiness during the remainder of the day and for several days following prevented further observation of the region. A spectroheliogram of the disk at G. M. T. 3^h 43^m, using the H line, reveals no other disturbance at this point, and very faintly shows the prominence itself beyond the limb.

The various stages of development of the prominence are well illustrated by the first four exposures, which are reproduced in Plate X.

At first it was firmly rooted to the disk but the connection gradually disappeared, leaving it floating free. It seems plain that the peak of the prominence, as seen in Fig. 1, toppled back to the disk forming the loop of Figs. 2 and 3. Careful comparison of the plates shows that the single spike of Fig. 1 coincides with the center of the loop of Fig. 2, and further that on Fig. 1 there is a very faint arch south of the spike which agrees well in position with the upper arm of the loop. It is probable, however, that the spike bowed to south when the peak fell back. Between Figs. 2 and 3 there is also a decided change. The falling arm of the loop has descended a considerable distance and the force which was to comb the crest of the arch into the long streamers of Fig. 4 was already active. There was a perceptible movement toward the south during the various changes. The two small promi nences at position angles 125°.9 and 146°.9 form good points of reference in comparing the several plates.

It would have been interesting to have some intermediate exposures, as the transformations could then have been followed with greater certainty. The need of obtaining many successive exposures at short intervals of these protean structures is strongly emphasized. It is possible with the Rumford spectroheliograph to make exposures on prominences around the whole disk at intervals of three minutes, and for single prominences the exposures may follow at intervals of less than a minute. For example, on June 8, 1907, I made such a series of exposures upon a group of quiescent prominences at the south limb of the sun, obtaining twenty exposures scattered through an hour. Six of these were made within five minutes. I hope that I may soon have an opportunity to make a series upon a large, rapidly changing prominence.

YERKES OBSERVATORY
September 1907

ORBIT OF THE SPECTROSCOPIC BINARY μ *SAGITTARII*

By NAOZO ICHINOHE

This star ($\alpha = 18^h\ 8^m$, $\delta = -21°5'$; Mag. $= 4.1$) was twice observed by Mr. Wright with the Mills spectrograph, on June 19, 1899, and May 30, 1900, and the results show the velocity in the line of sight -75 and -76 km, respectively. Hence the variability of the radial velocity was not detected by him; and Professor Campbell included the star among examples of stars with large radial velocities.[1] In the course of their observations of stars of the *Orion* type, the variability of the velocity was soon discovered[2] by Messrs. Frost and Adams at this observatory, the velocities on April 15 and April 29, 1904, giving a range of variation of 80 km.

μ *Sagittarii* is a multiple star, having five companions whose magnitudes range from 9.2 to 13. Of course the variation of the radial velocity relates only to the principal star. The proper motion of the star was thoroughly investigated by Professor Auwers, as it is contained in his catalogue of the fundamental stars, and it is $0''.027$ in the direction of the position angle $280°.7$. The velocity of the center of inertia of this binary in the line of sight is -7 km per second, as we shall see later. Though we know the proper motion of the star on the celestial sphere as well as the radial velocity, yet we do not know the absolute amount of the motion in space, since the annual parallax of the star is unknown.

The star is included in the *Draper Catalogue* where the spectrum is stated to be of the F type, and the photographic magnitude is 4.23 according to the same catalogue. This is also included in Miss Maury's catalogue and the spectrum is classified under group VI *c*. The following statements will sufficiently describe the characteristic points of the spectrum contained in the region from λ 3900 to λ 4900. The hydrogen lines are all narrow compared with the stars in the foregoing groups, and very well defined. The calcium lines K and H are pretty strong. The helium lines are rather faint; the lines $\lambda\lambda$ 4009,

[1] *Astrophysical Journal*, 13, 99, 1901.

[2] *Ibid.*, 19, 351, 1904.

4024, 4121, and 4144 are all feebly impressed, still we can recognize them easily; λ 4388 is very well seen; but the strongest helium lines in this star are λλ 4026 and 4472, which are quite strong and very well measurable. The silicon lines λλ 4128 and 4131 are also quite strong and their intensities are just comparable with those of the strongest lines of helium. We can also clearly see the carbon line λ 4267. The magnesium line λ 4481 is quite distinct. Besides this, the lines which can be measured with accuracy are the pair of silicon lines, $H\gamma$, λλ 4388 and 4472, and also the carbon line. Many faint metallic lines may be recognized, especially those of iron and titanium.

The normal wave-lengths of the lines which have been used for the determinations of the radial velocities are as follows. In this table *n* denotes how many times the corresponding line has been used for the star, the whole number of the measured plates being 21.

Element	λ	*n*	Element	λ	*n*
Ca.K.....	3933.825	1	*Cr*.....	4385.144	1
He.....	4009.417	1	*He*.....	4388.100	16
He.....	4024.136	1	*Cr*.....	4465.519	1
He.....	4026.370	6	*He*.....	4471.676	21
$H\delta$.....	4101.890	13	*Mg*.....	4481.400	21
V.....	4111.940	1	*Fe*.....	4508.455	1
He.....	4121.016	3	*Fe*.....	4549.642	2
Si.....	4128.211	13	*Fe*.....	4584.018	2
Si.....	4131.047	13	*Ti*.....	4590.126	1
He.....	4143.919	3	*He*.....	4713.308	2
C?.....	4267.301	4	*Ti*.....	4856.203	1
$H\gamma$.....	4340.634	21	$H\beta$.....	4861.527	8

From the table we see that the only lines which were used for all the plates are $H\gamma$, λ 4472 and λ 4481. The lines $H\delta$, λ 4388, two lines of silicon, and a helium line λ 4388 were the ones which are used more frequently than the others.

The following journal of observations for the star requires little explanation. The temperature is that indicated by the thermometer within the outer temperature-case of the spectrograph. In some cases, the readings of the temperature at the beginning and the end of the exposure were not exactly the same; in such cases, their mean was taken in this table. With regard to them, the differences were not so large that the radial velocities determined with such plates would be affected. The column before the last gives the initials

or name of the observers, where F = Frost, A = Adams, and B = Barrett. Here it is understood that Mr. Sullivan guided equally with the observers. The last column gives the estimate of the condition of the sky made by the observer, the first figure representing transparency and the second steadiness. The number 5 is assigned for the very best conditions.

The comparison spectra of iron and titanium were equally impressed by means of the spark at the beginning and the end of the exposures.

μ SAGITTARII

JOURNAL OF OBSERVATIONS

Plate	Date	G. M. T.	Exposure	Slit-Width	Temperature	Observer	Seeing
IB 311....	1904 April 15	$20^h\ 36^m$	43^m	0.038 mm	+ 0°.1 C	A	3; 2
323....	April 16	21 36	40	0.038	+ 4.0	A	3; 3
328....	April 29	20 59	42	0.051	+14.8	A	2; 2
335....	April 30	21 04	35	0.038	+16.6	F	3; 1
780....	1906 June 1	20 17	50	0.046	+18.4	F	3; 3
796....	July 9	16 56	74	0.046	+24.8	F	2; 3
805....	July 20	15 21	76	0.046	+27.2	B	2; 2
816....	July 27	14 54	60	0.046	+26.6	B	2; 2
819....	Aug. 10	14 55	70	0.046	+24.8	B	2; 3
828....	Aug. 20	14 45	76	0.046	+27.6	F	2; 3
835....	Sept. 5	13 58	72	0.046	+23.2	F	3; 3
841....	Sept. 10	14 15	60	0.046	+25.8	F	3; 3
858....	Sept. 21	14 02	75	0.046	+22.0	B	3; 3
870....	Oct. 1	13 33	76	0.046	+17.0	F	4; 2
882....	Oct. 19	12 33	60	0.059	+16.1	F	5; 3
1022....	1907 April 13	22 15	61	0.046	− 0.9	Fox	4; 2
1025....	April 19	21 53	62	0.046	+ 5.8	F	3; 2
1038....	April 22	21 21	110	0.051	+14.2	F	0–2; 2
1045....	April 26	21 56	60	0.051	+ 3.7	F	3; 3
1050....	April 30	21 56	50	0.051	+ 4.2	F	3; 4
1059....	May 10	21 47	50	0.051	+ 5.9	F	3; 3

The measurements as well as their reductions were made according to the same processes as those used by others in this observatory. Among 21 plates, the plates IB 311 and 323 were measured by both Messrs. Frost and Adams. The plate IB 328 was measured by Adams and IB 335 by Frost. The measurements of all the remaining plates were made by myself. The following table shows the results of the measurements, in which the mean values of the two observers for the first two plates were taken. The second column of the table gives

the Julian day, only one decimal place being retained because the accuracy of the radial velocities and the present knowledge of the period make further figures unjustifiable. The third column represents the observed radial velocities reduced to the sun. In this case, the round numbers of km were taken throughout. The next column n shows upon how many lines the result depends.

Plate	Julian Day	v	n	Phase	v_c	$v-v_c$
IB 311....	2416586.9	+46 km	5	$139^d.1$	+44 km	+ 2 km
323....	6587.9	+42	8	140.1	+40	+ 2
328....	6600.9	−34	4	153.1	−33	− 1
335....	6601.9	−34	4	154.1	−36	+ 2
780....	7363.8	−55	11	15.0	−51	− 4
796....	7401.7	−18	8	52.9	−19	+ 1
805....	7412.6	−10	10	63.8	− 9	− 1
816....	7419.6	− 4	6	70.8	− 3	− 1
819....	7433.6	+12	12	84.8	+11	+ 1
828....	7443.6	+32	7	94.8	+23	+ 9
835....	7459.6	+45	8	110.8	+48	− 3
841....	7464.6	+37	11	115.8	+56	−19
858....	7475.6	+65	11	126.8	+65	0
870....	7485.6	+58	7	136.8	+50	+ 8
882....	7503.5	−40	8	154.7	−38	− 2
1022....	7679.9	−21	9	150.9	−21	0
1025....	7685.9	−44	5	156.9	−45	+ 1
1038....	7688.9	−44	3	159.9	−53	+ 9
1045....	7692.9	−42	10	163.9	−60	+18
1050....	7696.9	−63	8	167.7	−63	0
1059....	7706.9	−62	7	178.1	−62	0

The period of the oscillation of velocities was investigated in August of last year by me and it was found that 180 days satisfied all plates used at that time pretty well. For this determination I owe very much to the results by Mr. Wright. This year six more plates were added, which enabled me to correct the period; and finally $180^d.2$ was taken as the value of the period. The fifth column of the above table was calculated by using this value of the period and J. D. 2414826.0 as the initial epoch. Then the velocities were taken as the ordinates and the phases were taken as the abscissas.

These being plotted upon cross-section paper, a curve was drawn through or near to these points so that the curve became as smooth as possible. Then the elements of the orbit were determined by the method of Lehmann-Filhés.

First of all, the radial velocity of the center of inertia of the system

was determined to be -7 km. Then the following values were obtained for the data necessary for the determination of the elements:

$A = 72$ km	$B = 57$ km
$Z_1 = 683$	$Z_2 = 1708$
$t_1 = 148^d.3$	$t_2 = 231^d.0$

These give the following elements:

$$U = 180^d.2$$
$$u_1 = 96°41'$$
$$\omega = 74°43'$$
$$e = 0.441$$
$$\mu = 2°.00$$
$$\text{or } \log \mu = 8.5425$$
$$a \sin i = 143{,}500{,}000 \text{ km}$$
$$T = 144^d.4$$
$$\text{or } T = 2414968^d.4$$
$$m + m' = \frac{3 \cdot 5 \odot}{\sin^3 i}$$

If we represent $a \sin i$ in terms of the mean distance of the earth from the sun, it will be 0.965. Therefore, a is quite comparable with that of the earth's orbit unless the inclination be quite small. I calculated the following, assuming various values of i:

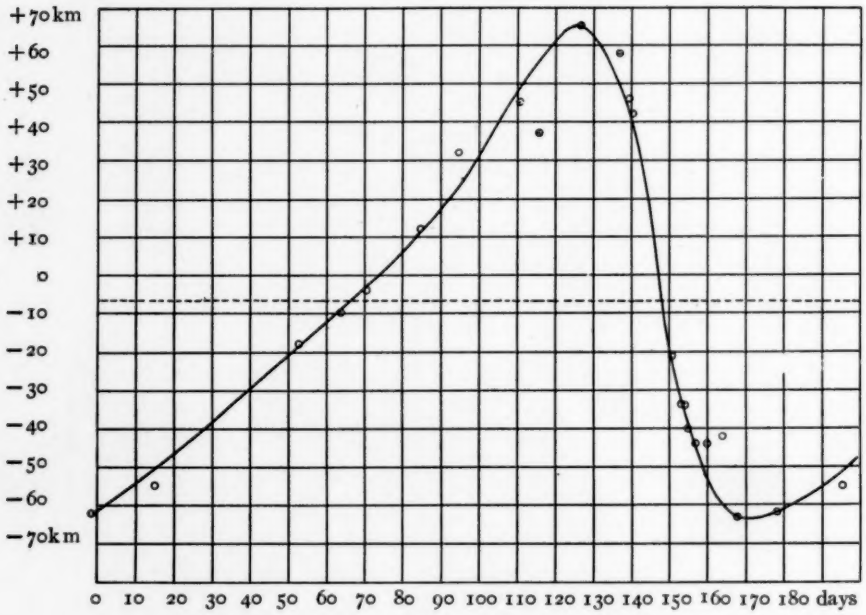
i	a	Astron. units
30°	287,000,000 km	= 1.930
45	202,900,000	= 1.365
60	165,700,000	= 1.114
75	148,600,000	= 0.999
90	143,500,000	= 0.965

With regard to the ratio of the masses of the both components I cannot say more than the above with certainty.

Next, using the above set of elements, I have calculated an ephemeris in order to see how closely these elements will represent the observations and the curve in the accompanying figure was drawn with these computed values. The centers of the small circles show the observed values.

The computed values were given in the column before the last in the above table. The deviations of the observations from the computations gives the values of the last column. The examination

of $v-v_c$ shows that the orbit represents the observations pretty well except for the two plates IB 841 and 1045. The former is a very good plate; still it gave too low a velocity; but when I consulted the original sheets of measurements, I found that the separate results for the different lines were not satisfactorily coincident. The latter is not a good plate; the comparison lines and star lines were very fuzzy.



COMPUTED VELOCITY-CURVE FOR *μ Sagittarii*, WITH OBSERVED POINTS

For this reason I measured these two plates again and found the following results:

IB 841	+43 km
1045	−42 km

As stated already, the two plates by Wright played an important rôle in the determination of the period of the star, but when the discussion was made, we found that the minimum velocity of the star is −63 km instead of −76 km; and the residuals become −12 and −14 km respectively. These are too large for accidental errors. Undoubtedly Wright's plates were obtained with the Mills three-prism spectrograph.

Our result rests entirely on Bruce one-prism spectrograms. According to the long experience, there is no appreciable systematic difference between the results by the Mills three-prism instrument and those by Bruce three-prism instrument. The question then is whether there is any systematic error between these results from the one-prism and three-prism plates, or whether the above residuals can be considered as merely accidental errors. At present my data are not sufficient to decide which assumption is preferable. It should be stated, however, that measures of control spectrograms of the Moon (four in number, made several years ago) by Mr. Adams and Mr. Frost indicate no systematic differences between three-prism and one-prism plates.

It is with great pleasure that I acknowledge my indebtedness to Professor Frost who suggested that I investigate the star and was interested in the work while I was carrying it on.

YERKES OBSERVATORY
August 1907

A GRAPHIC DETERMINATION OF THE ELEMENTS OF THE ORBITS OF SPECTROSCOPIC BINARIES

By KURT LAVES

So far there does not seem to have been offered a purely geometric proceeding by which the elements of the orbit of a spectroscopic binary can be determined. The two methods of Lehmann-Filhés[1] and Schwarzschild[2] combine certain geometric features with an analytic mode of solution.

Now it is evident from the geometric properties of the hodograph of conic sections described under Newton's law of attraction, that it will permit a very direct derivation of the formula for $\frac{dz}{dt}$ in terms of K, e, u, ω, and i as it is used in this work. Since the majority of the astrophysicists employ the original notation of Lehmann-Filhés, it seems best to adopt it in what follows. K, in our way of interpretation, is simply the radius of the hodographic circle. Since we are unable to determine the inclination i between the orbital plane and the tangential plane, we may as well assume $i=90°$; i. e., suppose that the line of sight is constantly contained in the plane of motion. This permits us to avoid the elliptic projection of the hodographic circle on the plane of sight which passes through the line of nodes. We shall see that after the velocity of the center of gravity γ and the period U has been found from the curve of oscillation (to use Hartmann's very appropriate abbreviated term), we determine from A and B the radius K of the hodograph and $\gamma+Ke\cos\omega$, the latter term being the perpendicular distance from the center of that chord on which the occupied focus of the ellipse is located. It will be remembered that A is the maximum positive and B the maximum negative velocity in the line of sight. By Schwarzschild's ingenious device we next obtain the time T of periastron passage and the $\frac{dz}{dt}$ component of the periastron above the S-axis. This gives us at once

[1] *Astronomische Nachrichten*, **136**, 17, 1894.

[2] *Ibid.*, **152**, 65, 1900.

in the hodographic circle the position of the diameter on which the focus of the ellipse is located, and we obtain this point itself as the intersection of this diameter and the chord previously constructed. Thus we find ω and e, since the distance from the center to focus

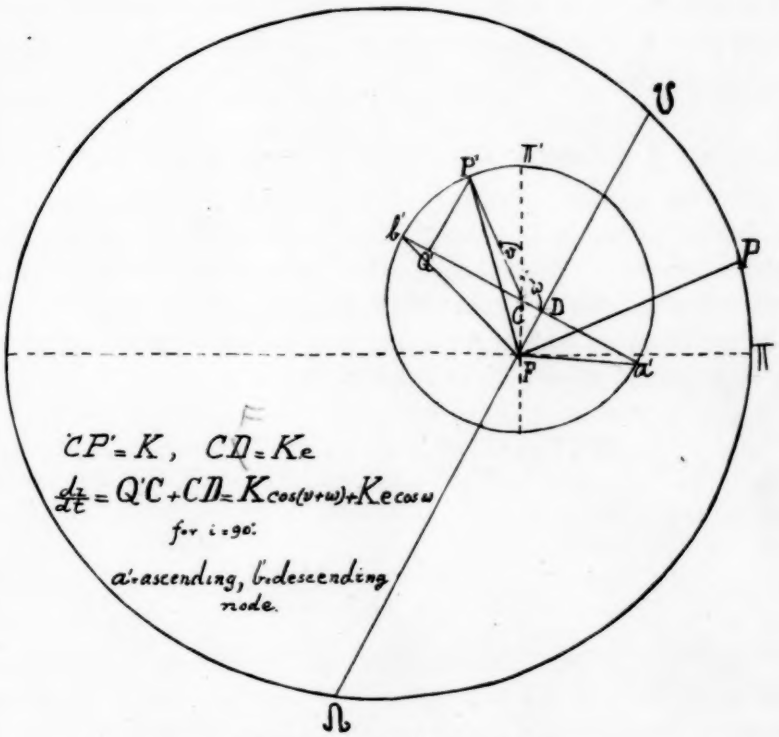


Diagram for case of χ *Draconis*

is $K \cdot e$. It seems that this procedure for finding ω and e will hardly ever lead into serious difficulties, so that the computer of orbits need not look for special precepts, as to the use of one or the other methods by which these quantities can otherwise be found.[1] It will prove to be very advantageous not to employ too small a unit for drawing the oscillating curve. The curve for χ *Draconis* by Wright[2] appears to be about

[1] See W. Zurhellen, *Astronomische Nachrichten*, **173**, 353; **175**, 245, 1907.

[2] *Astrophysical Journal*, **11**, 132, 1900.

what is desired; here the diameter of the hodographic circle comes out to be 2.5 inches.

Every point P on the ellipse (see figure) has its corresponding point P' on the hodograph, so that FP' (where F is the principal focus of the ellipse) gives the velocity of P both in magnitude and direction. The center C of the hodograph is at a distance $K \cdot e$ above F on the latus rectum, where K is the radius of the hodograph and e the eccentricity of the ellipse. The radius K is $= \frac{f^2}{c}$, where the constant of attraction f is either $\kappa \cdot \sqrt{m+m'}$ or $\frac{\kappa \cdot m'^{\frac{3}{2}}}{m+m'}$. The first value is used when we deal with the motion of a particle m with respect to a particle m', while the second value pertains to the motion of m with reference to the center of gravity of m and m'. The true anomaly v of the point P in the ellipse reappears in the hodograph at the center C, so that we write the expressions for the rectangular co-ordinates of P and P' as follows:

Point P

$$\xi = r \cdot \cos v \qquad (1)$$
$$\eta = r \cdot \sin v$$

Point P'

$$\xi' = K \cdot \cos (90^\circ + v) \qquad (1')$$
$$\eta' = K\,[e + \sin (90^\circ + v)].$$

The positive ξ-axis points toward the periastron point Π, and the positive η-axis to a point of true anomaly 90°. It is important to notice that if P_1 and P_2 are two points in the ellipse at the end of a focal diameter, then the corresponding points P_1' and P_2' will be at the extremities of a diameter of the hodograph.

The orbit of the binary intersects the tangential plane at F in the line of nodes. A system of three rectangular axes is so constructed that the positive z-axis coincides with the normal to the tangential plane away from the observer; the positive x-axis points to the ascending node of the particle m, and the positive y-axis to a point 90° ahead in the motion of the particle. To find the ascending node, we will say that it is that one of the two nodal points, where m attains positive z-components. Since we have put $i = 90$, we derive from our

figure at once the expression for the component of the velocity V which is perpendicular to the line of nodes. We obtain

$$\frac{dz}{dt}=K(e\cos\omega+\cos u) \qquad (2)$$
$$\omega=a'C\,\Pi'$$
$$u=a'CP'$$

(a' and b' are the points in the hodograph which correspond to the ascending and descending node respectively).

Equation (2) is obtained by projecting the velocity $V=FP'$ on the line $b'a'$. It is evident that if we elevate our line of nodes by $Ke\cos\omega$, so that we refer the velocities to the diameter parallel to the line of nodes, we shall find that the observed velocities should with respect to $u=v+\omega$ fulfil the sine-curve if no perturbations prevail in the system. If we call this diameter the "nodal" diameter we can say that points which differ by 180° in their true anomaly will have equal and opposite radial velocities with respect to the nodal diameter. Schwarzschild's clever procedure to find the time T of periastron passage makes use of this very property. It seems not to be used as extensively as it should be; it is both a very reliable and quick mode of finding T. In the majority of cases the velocity γ of the center of gravity is already determined when the curve of oscillation is published, otherwise this must be done in the usual fashion.

Explaining now further the mode of proceeding by the hodographic curve, we assume that γ, T, and U (period) have been obtained. From the maximum and minimum velocities we find

$$K=\frac{A-B}{2} \text{ and } Ke\cos\omega=\frac{A+B}{2}. \qquad \begin{matrix}\text{(It is assumed that } A\\ \text{and } B \text{ are corrected for } \gamma.)\end{matrix}$$

We therefore construct a circle with radius K and measure off $Ke\cos\omega$ from the center C along a diameter; at the end of this distance we erect a perpendicular on the diameter. On this chord the focus F must be located. To obtain F we enter the curve of oscillation and measure the $\frac{dz}{dt}$ component of the periastron above Schwarzschild's axis of symmetry (which corresponds to the nodal diameter above). At the perpendicular distance equal to this ordinate we draw in the

hodograph a chord parallel to the nodal diameter. Of the two points of intersection but one will fulfil the condition to fall on the proper arc between ascending and descending node. The ambiguity whether the periastron point in the hodograph lies above or below the nodal diameter is easily settled. Let us call "periastron arc" that arc of the ellipse which terminates at both ends at the latus rectum and contains the periastron. Then we see that whenever $A>B$ the ascending node will be on the periastron arc; for $A=B$ the ascending node will be on the latus rectum of anomaly 90° if the time from the ascending node to the descending node is longer than that from the descending to the ascending node; if not, the ascending node is on the latus rectum where $v=270°$. From the curve of oscillation we can therefore settle the ambiguity in the position of the periastron without difficulty. Whenever $A=B$ we have either a circular orbit or an elliptic orbit with $\omega=\begin{cases}90°\\270°\end{cases}$. After the periastron point has been located on the hodograph we draw the diameter which passes through it. This diameter cuts the nodal chord in F, and measuring CF in terms of the radius K with a finely graduated scale (100 parts to an inch is a very suitable subdivision), we obtain e, and by measuring the sine-line of the periastron point we obtain ω.

Finally we make use of the equation of areas $r^2\,dv=c\cdot dt=K\cdot p\cdot dt$, and integrating over the entire ellipse, we get $\pi ab=K\cdot p\cdot U$, or $a=\frac{2K}{\mu}\sqrt{1-e^2}$, where $\mu=\frac{2\pi}{U}$. We must not forget, that to make our formulas comparable to those generally used, we should replace K by $K\sin i$ whenever this quantity enters.

It will not be out of place to show by an example how very rapidly this geometric proceeding leads to an evaluation of the elements. After Vogel's thorough investigation of the orbit of *β Aurigae*[1] it seems superfluous to adhere to the set of observations by which Rambaut, Lehmann-Filhés, and Schwarzschild have tested their methods. I have selected therefore instead the stars *χ Draconis* and *η Aquilae*, which have been so ably discussed by Wright.

I. *χ Draconis*.[2]—The curve of oscillation on page 132 seems to be drawn with extreme accuracy. In order to avoid errors by using

[1] *Astrophysical Journal*, **19**, 360, 1904. [2] *Ibid.*, **11**, 131, 1900.

a self-made subdivision of the scale contained in the diagram, all measurements were made with a metallic scale of 100 graduations to an inch.

By actual measurement we find

$$A = +321 \quad \text{(not yet corrected for } \gamma)$$
$$B = +\ 72$$
$$K = \frac{A-B}{2} = 124.5$$
$$\gamma + Ke\cos\omega = \frac{A+B}{2} = 196.5.$$

Wright gives $\gamma = 32.2$ km; this on the scale employed is equal to 220. Hence $Ke\cos\omega = -23.5$; the negative sign indicates that Schwarzschild's axis is 23.5 below the γ-axis given by Wright. By Schwarzschild's method of reflection and translation by $U/2$ along the time-axis we obtain the following four points of intersection of the original curve and its superimposed image:

1899 March 9.2
April 15.6
July 26.9
Sept. 4.2 .

From the condition that the interval of time between a pair of dates must be $140^d.5 = \frac{U}{2}$, and at the same time that the corresponding velocities above Schwarzschild's axis should be equal and of opposite sign, we single out March 9.2 and July 26.9 as the only possible pair of dates. Of these July 26.9 is the periastron point, because the curve of oscillation is here decidedly steeper than at March 9.2. Since the periastron point is below the axis, its corresponding point on the hodograph must be below the nodal diameter. We next draw the hodographic circle with $K = 124.5$. Since $A < B$ (measured from the γ-axis), we see that the ascending node must be on the apastron arc of the ellipse, while the descending node is on the periastron branch. In the hodograph, periastron and focus lie on opposite sides of the center of the hodograph; since Π' is near to b' we must draw in the hodographic circle the chord at a central distance of $Ke\cos\omega = 23.5$ *above* the center. Next we measure off $56.5 = \frac{dz}{dt}$

of the periastron and find the point Π' on the hodograph. The diameter through Π' meets the nodal chord in F. We measure off by the scale $CF=53$; therefore $e=\frac{53}{124.5}=0.426$. Similarly $\sin(\omega-90)=\frac{60}{124.5}$ $\therefore$ $\omega=118°\,49'$. When we compare the values of the elements derived by our method with those obtained by Wright after the procedure of Lehmann-Filhés, we have:

	Wright	Laves
T	1899, July 27.0	1899, July 26.9
e	0.45	0.426
ω	114°.99	118° 49′.

Wright has derived a second set of elements by a method of least-squares solution, and his final values are

$$T=1899,\ \text{July}\ 28^{d}.3\pm0^{d}.5,$$
$$e=0.423\pm0.006,$$
$$\omega=119°\ 0'\pm1°.1.$$

It is rather remarkable that our first set of elements comes so very close to these improved values. The remaining elements, U and $a\sin i$, are in no way altered by my procedure and are therefore not quoted. It should be remarked that the time consumed in the determination of an orbit by this geometric method is very short indeed, and it is therefore well suited for a check-determination of orbits obtained by other methods. Moreover, the least-squares solution, which one is bound to use with well-determined stars, gains not a little by this graphical method.

II. *η Aquilae.*[1]—Figure 1 in Wright's paper, gives the curve of oscillation; from it we obtain by an analogous procedure with the one under (I):

$$A=+\ 37, \qquad \frac{A-B}{2}=K=77,$$
$$B=-117, \qquad \frac{A+B}{2}=Ke\cos\omega+\gamma=-40,$$
$$\text{From the figure} \qquad \gamma=-53.5.$$
$$\text{Hence} \qquad Ke\cos\omega=+13.5.$$

[1] *Astrophysical Journal*, 9, 60, 1899.

This axis we draw $+13.5$ parts above the γ axis. By Schwarzschild's method we obtain $T=6^{d}.24$ and the corresponding $\frac{dz}{dt}$ is 29. The periastron point is above the axis S; when we subtract γ from A and B we see that $A>B$, the ascending node is on the periastron branch. From the hodograph and the quantities mentioned we obtain

Laves		Wright
$e=0.44$		$e=0.47$
$\omega=67^{\circ}\ 53'$	as against	$\omega=65^{\circ}.79$
$T=6^{d}.24$		$T=6^{d}.176$

Wright's second determination by the method of least squares gives

$$e=0.489\pm0.014$$
$$\omega=68^{\circ}\ 55'\pm1^{\circ}.95$$
$$T=6^{d}.210\pm0.028^{d}.$$

THE UNIVERSITY OF CHICAGO
September 1907

DETERMINATION OF WAVE-LENGTHS OF LIGHT FOR THE ESTABLISHMENT OF A STANDARD SYSTEM[1]

By PAUL EVERSHEIM

The knowledge of the wave-length of light to a few thousandths of an Ångström unit is important for many spectroscopic and astronomical purposes. This accuracy is attainable with a large grating by interpolation between neighboring standards, but the standards themselves cannot be obtained with a grating, as the method of coincidences fails. Hence it is necessary in establishing a system of standards to use some one of the interference methods, of which that of Perot and Fabry seems to me to be the most practical. The decision of the International Solar Union (Oxford meeting) was followed that the standards should at most not be farther separated than 50 Å, and that numerical values should be determined as far as possible with an accuracy of 1/1000 of an Ångström unit.

The method may be described as follows. For producing the phenomena, use is made of a so-called "silvered air-stratum." This is obtained by setting up two plane-parallel glass plates, figured as accurately as possible, with a thin silver coat on one side, and then adjusted to accurate parallelism with the silvered surfaces toward each other. If a cone of converging homogeneous light then passes through the air-stratum so formed, the observer employing a telescope focused on infinity will see from the back side a number of concentric rings, a phenomenon of interference which is produced in the same manner as that of thin plates—as can be clearly seen from Fig. 1.

Let $S' \ldots . S$ be parallel rays of a beam of homogeneous light of wave-length λ, meeting the air-stratum at an angle of ϕ. The rays in part pass directly through the stratum, but another portion is reflected within it. If an incident ray then interferes with a reflected ray, it generally will have a difference of path with reference to the former, which we will call δ, and if δ/λ is a whole number, the two rays will reinforce each other. But since the incident rays are reflected very frequently, not only will the neighboring ray contribute its portion δ, but also the

[1] Translated from *Zeitschrift für wissenschaftliche Photographie*, 5, 152–180, 1907.

following will give 2δ, the third 3δ, etc., whence we see that an increase of the intensity is produced according to the degree of the reflection. This occurs only for the case mentioned when δ/λ is an integer, i. e., for a definite angle of incidence. The rays, however, are incident under all possible directions, and in the general case δ/λ differs from a whole number. But the slightest deviation will also ultimately lead to a difference of path on account of the great number of reflections, which is for an incident ray the value of $\lambda/2$, in which case extinction will result. It follows from this that only the rays which are so incident upon the air-stratum that they receive a difference of path of λ, 2λ, $\ldots . n\lambda$, can be reinforced by interference, while all the rest must be rendered more or less faint; in this process the larger number of rays will participate according to the frequency of the reflection and the degree of silvering; whence we reach the further conclusion that the interference phenomena will increase in distinctness in the same measure.

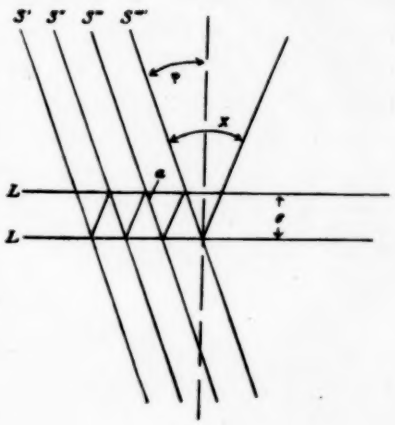


FIG. 1

A number of rings separated from each other by broad dark intervals may now be seen in a telescope focused on infinity, since the incident rays are symmetrically distributed about the axis of observation. On bringing the paths closer together, one after another of these rings moves in toward the center and there vanishes until the whole phenomenon has disappeared at the distance zero; and conversely, one ring after another becomes visible as the paths are separated. The first ring appears when the difference of path of two interference rays has become equal to a wave-length of the particular kind of light, the second at a difference of path of two wave-lengths, etc., and we may accordingly speak of a first and second order of the rings, and so on, which we will designate with the letter *P*.

If two beams composed of homogeneous light are incident simultaneously, but of which the one has only rays, say, of the red line of cadmium, while the other has only rays of the green cadmium line, then we should observe two systems of rings; then if the distance of the path is sufficient, it is not difficult to recognize that a red ring is superposed upon a green ring, at definite intervals so that they coincide. If an exact coincidence is observed, that is, one in which a red ring precisely covers a green one, and if we know the order P and P' of the rings at this place, then we shall have the relation

$$P\lambda = P'\lambda',$$

where λ and λ' denote corresponding wave-lengths. If λ is known, the unknown wave-length may be computed by the formula

$$\lambda' = \frac{P\lambda}{P'}.$$

For determining the number of the order Perot and Fabry constructed a so-called interferometer, an apparatus permitting the two silvered plates to be displaced parallel to each other. In principle their measurement consisted simply in departing from zero to determine the number of the order by counting, an exceedingly wearisome task, but one, however, which Perot and Fabry nevertheless carried out for some of the mercury lines.

By modifying the interferometer Perot and Fabry then constructed an apparatus which was far more convenient in operation and which above all was unaffected by the unavoidable disturbing vibrations. It was with such an apparatus at a definite separation of the two silver strata that the author made his observations, and it is probably necessary to describe the apparatus pretty fully.

So far as the theory of the phenomena is concerned, the matter is very simple and leads very quickly to the method which must be used for definitive computation. Let us refer again to Fig. 1 and assume that we are observing the rings of the green cadmium line in the manner described above.

The separation of the plates, hence the thickness of the air-stratum, may be called e, corresponding to a wave-length λ, when we shall obviously have

$$2e = P\lambda; \text{ hence } \lambda = \frac{2e}{P},$$

this applying to the case of vertical incidence of the rays, hence here for the center. But if the incident rays make the angle ϕ with the normal, we shall have

$$P=\frac{1}{\lambda}\left(\frac{2e}{\cos\phi}-a\right),$$

or, since $a=2e\tan\phi\sin\phi$,

$$P=\frac{1}{\lambda}\left(\frac{2e}{\cos\phi}-\frac{2e\sin^2\phi}{\cos\phi}\right),$$

which becomes

$$P=\frac{2e}{\lambda}\cos\phi\,.$$

We can develop the cosine function in a series, and neglect the higher powers, as ϕ is very small. We then arrive at the expression

$$P=\frac{2e}{\lambda}\left(1-\frac{\phi^2}{2}\right). \qquad (1)$$

If a beam of a different wave-length λ' is incident, we get similarly

$$P'=\frac{2e}{\lambda'}\left(1-\frac{\phi'^2}{2}\right). \qquad (2)$$

The last two equations give us the means for computing the unknown wave-length λ', after we have made one experiment each for the same separation of the plates with comparison light of the wave-lengths λ and λ', and have measured the angles and have in any way whatever obtained the number of the orders P and P'. If in place of the angle of incidence ϕ, we substitute the angle actually to be measured, $x=2\phi$, or $x'=2\phi'$, then the expression in brackets will take the form

$$1-\frac{x^2}{8} \quad \text{and} \quad 1-\frac{x'^2}{8}\,.$$

Taking account of this in equations (1) and (2), and considering that the high powers of x may be neglected on account of the smallness of the angle, we get the simple equation

$$\frac{P'}{P}=\frac{\lambda}{\lambda'}\left(1+\frac{x^2}{8}-\frac{x'^2}{8}\right), \qquad (3)$$

or

$$\lambda'=\lambda\frac{P}{P'}\left(1+\frac{x^2}{8}-\frac{x'^2}{8}\right). \qquad (4)$$

As source of the comparison light, the wave-length of which must be very accurate, we are fortunately in possession of data of an extremely high degree of precision, thanks to the excellent measures of Michelson.[1] These are the lines of the cadmium spectrum,[2] of which the red line is better adapted for comparison, while the green one is commonly more convenient for use. The details will be discussed later.

The determination of the numbers of the order appears at first somewhat difficult. It is, however, easy to compute P' in case P has been previously determined, and a series of measures has been made for the appropriate wave-lengths. The wave-length λ' which is to be measured is in general accurately enough known so that P' can at least be approximately computed from equation (3); that is, it may be established that the error amounts to only a few units of the second decimal of a whole number, the number of the order, which can be only a whole number; the actual value must then be the nearest whole number to this value. The determination of P is, however, not so simple. For this Perot and Fabry used their interferometer, but the mode of measurement, which I shall not discuss in detail here, offers the same difficulties which arise in determining the wave-length by the method of coincidences described above and in counting the rings; and it seems to me that the method suggested by Lord Rayleigh[3] for P is very much more simple. This method is as follows.

If the silvered air-stratum of a definite thickness has been arranged as a standard with which the measurements are to be undertaken, a series of experiments is first made with various kinds of light of known wave-length. The distance of the silver stratum can now be measured to an accuracy of $\frac{1}{100}$ of a millimeter, whence is obtained an approximate value for the number of the order from $P=\frac{2e}{\lambda}$. For instance, if $e=3.19$ and $\lambda=5085$, we obtain the number 12550; from this we know that the value for P, if not precisely this number,

[1] "Détermination expérimentale de la valeur du métre en longueurs d'ondes lumineuses," *Travaux et Mémoires du Bureau international des poids et mesures*, 11.

[2] Fabry has expressed his readiness to test Michelson's value by another method; and the work is now nearly completed.

[3] "Some Measurements of Wave-length with a Modified Apparatus," *Phil. Mag.*, 11, 685, 1906.

certainly must lie in its neighborhood; and if e has been measured pretty closely, it may be assumed with safety that P is to be found above 12550.

We therefore proceed from this number and compute by equation (3) P' for a wave-length λ', after we have determined x or x' by experiment. If no whole number is found for P', the same computation will be made at 12551, etc., until we come to a value where P' is a whole number. This, however, might be an accident, and in order to be entirely certain, we take perhaps five wave-lengths, computing for each the corresponding number of the order after the experiments have been made always with the same source of light. The values are then collected in a table, and ultimately must form a series consisting solely of whole numbers. Of course the measurement here need not be so exact as is necessary in the determination of wave-lengths; and we may content ourselves with a single photograph and measurement, even if slight deviations now arise from this cause, which, however, are not of importance. I give below a series from the table for determining P.

$e=3.19$ mm

Cd			*Hg*			
Red	Green	Blue	Yellow 1	Yellow 2	Green	Violet
9897.02	12529	13275.02	11004.04	11044.0	11669.04	14620.0

For the comparison line *Cd* 5085.224 we therefore get, without any uncertainty, $P=12529$ for a separation of the plates $e=3.19$. The computation of the number of the order, and of course also of the wave-length, presumes the knowledge of the angle x or x' at which a ring is formed, for the comparison light as well as for the light under investigation. These values can be obtained directly in degrees, minutes, and seconds if the pair of plates is mounted on the table of a goniometer, and if the measurements are made with telescope and cross-hairs in the ordinary way. This method furnishes usable results if the observer has a good instrument, and the lines concerned are very bright. But in most cases the observations deal with a very faint light so that direct measurement is exceedingly wearisome and unreliable. The photographic method is therefore to be preferred, and indeed its use is practically required.

The measurement proper is thus somewhat altered, as we shall now explain.

Each element of the ring is formed by parallel rays which are united by the lens L in their focal plane, that of the photographic film. The angle for a ring evidently has its vertex in the principal point of the lens. Therefore if we know the focal length R of the lens, and measure the diameter of the ring D, we shall get, remembering that x is very small,

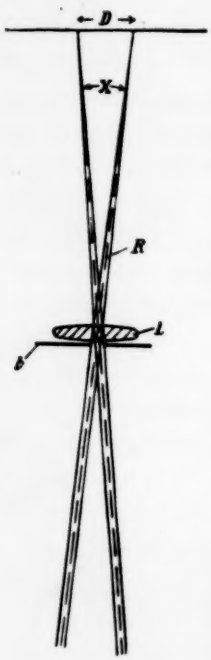


FIG. 2

$$\tan x = x = \frac{D}{R}.$$

Thus for the final computation, equation (4) takes the form

$$\lambda' = \frac{P\lambda}{P'}\left(1 + \frac{D^2}{8R^2} - \frac{D'^2}{8R^2}\right). \qquad (5)$$

ARRANGEMENT OF EXPERIMENT AND MODE OF MEASUREMENT

As already mentioned, for the computation of wave-length by the interference method, a source of light is necessary for a comparison which contains a line that can be accurately determined and has been accurately measured. As we have already seen, such lines occur in the cadmium spectrum, and for comparison the green line which was determined by Michelson was selected. In his experiments Michelson used the tube which bears his name, a somewhat modified Geissler tube, which was provided with a few crystals of cadmium metal, and which when excited in the usual way yielded the spectrum of cadmium. The lines are, however, rather faint and are not well adapted for work with the interference apparatus. A most disagreeable fact is that such tubes very soon become inactive and after only a few experiments they may crack at any place whatever. It was very fortunate for me, then, that a cadmium arc lamp from the quartz works of the firm Heraeus in Hanau was recommended to me which left nothing to be desired in the way of brightness and purity of the spectrum. This

lamp consists of a ∩-shaped quartz tube, the two arms of which are filled with cadmium in the lower wider portion, which is in metallic contact with the nickel-steel electrode fused into the quartz wall. The arms are cooled with water and the arc passes over the upper closed part. The lamp is kindled by an induction spark after previous warming, and it requires a current strength of four amperes. The arc is best maintained at a tension of 220 volts. It is necessary to leave the air-pump attached, as it is often required during its use to again pump out air. If the lamp is well mounted, these disturbances need not be feared, and it will render very excellent service. I have already burned this one several hours without having to change

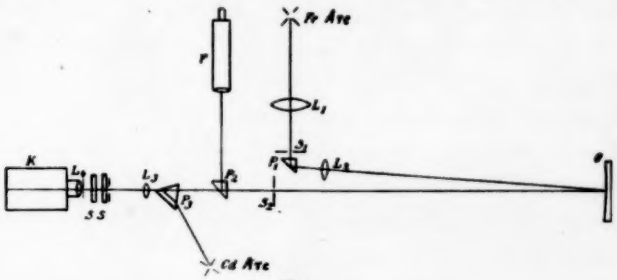


FIG. 3

anything about it. The objection can be raised, it is true, that the lines used of the spectrum produced under these altered conditions are not identical with those of Michelson's tube. A more accurate test which I applied to the red and green lines, however, indicated that a difference in the wave-length could not be established, so that the lines of the arc lamp could be used for comparison without fear.[1]

All of the observations were made by means of photographic plates. This procedure has the advantage over the direct observation of preserving by a longer exposure phenomena which are in part very faint; then one can always check up his earlier measurements on the plates, and above all can undertake investigations of the spectrum in the invisible portion. The arrangement of the apparatus can be seen from Fig. 3.

[1] See Michelson, *loc. cit.* He tested the *Cd* lines under the most varied conditions, such as alternation of pressure, temperature of tube, and strength of current. He made experiments with old tubes, compared these with new ones, took commercial cadmium, used chemically pure cadmium; he always obtained the same values for the wave-lengths.

The iron arc burning with 5 to 6 amperes casts its light by means of the lens L_1 on the slit S_1. The beam is then passed by the totally reflecting prism P_1 through the lens L_2 on a medium-sized Rowland concave grating G, so that a sharp iron spectrum is obtained in the plane of the slit S_2. For purposes of orientation in this spectrum the telescope S is employed, which is set on the slit S_2, after the prism P_2 has been brought to the proper place, and then is brought into coincidence with the cross-hairs. S_2 is now removed and a rotation of the grating makes the line under investigation come into coincidence with the cross-hairs as the slit did previously. If we then bring the slit S_2 back to its former position and remove P_2, we can project by the lens L_3 an image of the illuminated slit through the silvered air-stratum between the plates *ss* on the small diaphragm of camera *k*. The incident rays of the wave-length in question interfere and we obtain from the cone-shaped beam in the focal plane of the quartz lens L_4 (f=circa 15 cm) a system of rings which can be photographed. The comparison line is obtained by means of the prism of carbon bisulphide P_3 from the cadmium arc as source, and it is brought to the correct position by turning the prism.

For the iron arc I employed a lamp with hand regulation, and at 220 volts tension made the arc as long as possible. When this was the case and the lamp was burning quietly, I obtained very good plates. It is true that such an arc sorely tries the patience of the observer, burning often for a good quarter of an hour without disturbance, whereupon it suddenly makes the wildest leaps and is only with difficulty quieted down. I have also used the iron arc in a vacuum, but the flickering was even worse here.

The movable portion of the whole apparatus was adapted to the requirements so that with suitable tracks and slides the alterations desired could be undertaken in the dark and the slit and prisms brought to the proper place.

THE CONDITIONS FOR PROPER OPERATION

1. THE PARALLELISM OF THE AIR-STRATUM

As we have already frequently seen, our experiments require a silvered air-stratum of definite thickness. For this we use two glass

plates, and in order to set these up at a definite distance, we need separators. These are applied at three points at the edge of the plates, which are then pressed together by rings, as may be seen from Fig. 4, in which *a* shows the plan, and *b*, the section, exhibits the mechanism. The stiff iron casting *E* has an aperture in the center, at the edge of which a flange *w* is turned. After one plate is placed in this flange, the small balls *d* which separate the plates are put in place and covered with the other plate. A spring *f*, adjustable lengthwise and also capable of being clamped, is attached to each of the three

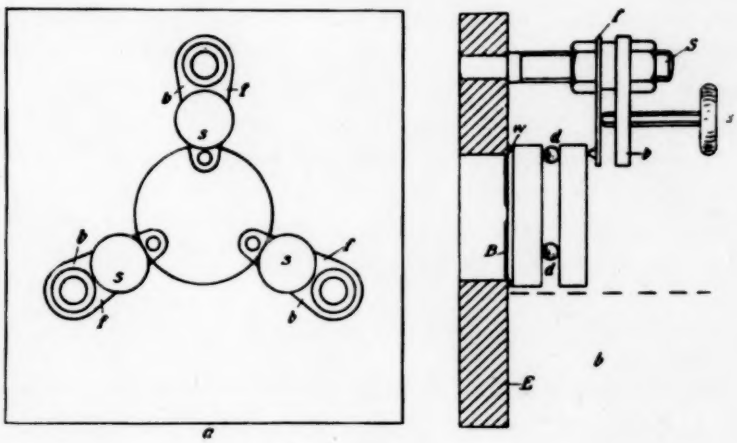


FIG. 4

bolts *S*. The piece *b* carries the screw *s* by which the pressure on the spring can be adjusted independently of the position with respect to the bolts.

The balls were as hard as glass and figured with an astonishing accuracy. As soon as the apparatus is put together, the interference rings are seen in homogeneous light, which can be accurately adjusted by a slight pressure of the ring *f* by the screw *s* in bringing the two plates into accurate parallelism. This adjustment is in fact the principal problem of the experimenter. We saw above that the computation of the wave-length required a knowledge of the angle x at which the ring is formed. The slightest change in the separation of the plates also alters the diameter of the ring and with it the angle x.

Therefore if the air-stratum is wedge-shaped, however slightly, this will change, if the incident rays are displaced in the direction of the wedge. When work is done with different sources of light it is readily possible that the effective rays do not always strike the pair of plates at the same place, whence errors must result from a wedge-shaped air-stratum.

This peculiarity, that the diameter of a ring changes with the slightest change in the distance of the plates, offers at once the means of adjustment, for which purpose the plate-holder of Fig. 4 is mounted on a stand which permits it to be displaced in both a horizontal and a vertical direction. Thus we can make the rays pass through very different parts of the plates, and can vary the pressure of the spring *f* until no variation in the size of the ring is longer perceptible in a region of about 3 cm diameter. But in order to render harmless anything still erroneous in the adjustment and any casual unevenness of the silver stratum, a diaphragm with an aperture of 3 mm is in the different experiments placed across in front of the plate; similarly a diaphragm of 1.5 mm aperture is placed in the camera lens.

2. THE UNIFORMITY OF THE SILVER FILM

The silvering of the plates must be effected with the greatest of care and any irregularities of thickness, and cloudiness must be avoided or at least reduced to a minimum, as otherwise the same errors would arise as are shown under 1. The silvering must be also carried to a certain definite degree; if it is too heavy, then too little light will penetrate and the phenomena will be indistinct; while if it is too weak, we shall have too few reflections and the rings will be lacking in sharpness. A correct density of silvering is determined by experiment.

3. ALLOWANCE FOR THE ERRORS OF THE LENS

The diaphragm *b* provides that all of the rays pass through the center of the lens *L* (Fig. 2). The distance of the lens from the photographic film is constant for all the plates, so that we have a mean focal length *R* remaining always the same. Mathematically considered, light of only a definite wave-length can then be sharply focused, but

in practice the differences are so small as to be inappreciable, and we may also simplify in this respect without fear of the consequences. A test of the result shows that a good scale is sufficient for the determination of R and it will be enough if the tenth of a millimeter is accurate. Moreover, it would be useless to attempt a greater accuracy, since the distance of the photographic film may well vary during experiments through a tenth of a millimeter.

4. CORRECTION FOR CHANGE OF TEMPERATURE

Temperature changes can produce a very disturbing effect during the measurements; and the change of a tenth of a degree will, on account of the expansion of the steel balls, produce a very perceptible alteration in the diameter of the ring. Inasmuch as it is difficult to avoid temperature variations within this range, we must at least take care that the experiment is made when the temperature is either rising or falling with the greatest possible uniformity. An exposure to the source of the comparison light will then be made before and after the exposure of the line to be measured, and in the computation use will be made of the mean value from the two exposures.

5. TEST OF THE CHANGE OF PHASE

Equation (5), for the computation of the wave-length λ', assumes that the separation of the plates or the thickness of the silvered air-stratum is the same for λ' as for λ. This is, however, not the case, for the distance varies from wave-length to wave-length, for the well-known reason that according to the color a ray of light will penetrate to a greater or less depth in reflection from the silver films. Although this is excessively small even for the rays that penetrate the farthest nevertheless the fact demands attention for precise measures; indeed, the thorough control in this respect is one of the principal problems in the measurement.

Experiment shows that the penetration into the silver film becomes greater with decreasing wave-length. We can assume that for the source of comparison light, in the case before us for the green cadmium line, the reflections take place for a distance compared with which the shorter waves all have a larger distance, the longer waves, on the

contrary, a smaller distance, just as if we were actually to change the distance correspondingly.

If we now determine the number of the order P of a ring[1] of light we must obtain a whole number; but if we undertake the same measurement for the red cadmium line, we shall observe the ring at a *smaller* distance; and since the number of the order also increases with the increasing distance, there must be added to the number of the order of the red line a small fraction ϵ, in order to make it refer to the greater distance, as is necessary. Conversely we shall have to subtract a corresponding amount for the shorter waves.

The absolute amount ϵ of this fraction cannot change if the separation of the plates is varied, inasmuch as the depth of the penetration

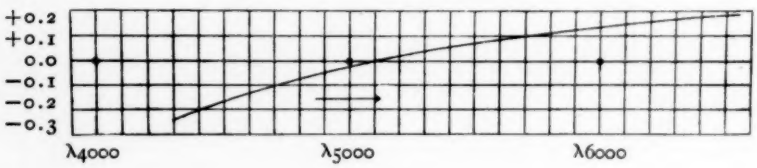


FIG. 5

into the film does not depend upon it. Therefore, if we make an exposure at a distance of the plates of 5 mm for the red cadmium line, and compute the number of the order, we shall obtain the value $P' + \epsilon$. If we repeat the experiment for a distance of 3 mm, we must get the value $P_0' + \epsilon$, where it is assumed that we accurately know the wave-length of the red cadmium line. If this is not the case, we may form two corresponding equations for the two different separations of the plates and thence compute the unknown quantities λ' and ϵ.

This peculiarity of the silver film now under discussion therefore requires for the definitive computation of the wave-lengths a table of corrections, or better a curve on which the change of P with the wave-length is graphically represented. In Fig. 5 the wave-lengths are drawn as abscissae, and the corresponding fraction ϵ of the number of the order as ordinates; here the change of phase ϵ for the green comparison line is placed equal to zero.

[1] In order that equations (4) and (5) shall remain correct, we may use in the measurement only the smaller rings situated at the center.

COMPUTATION OF THE RESULTS OF THE EXPERIMENT

Preliminary Remarks.—In what follows R denotes the mean focal length of the camera lens, D the diameter of the ring of the comparison light of the green cadmium line λ 5085.824, D' that of the wave-length to be measured. For determining the diameter I employed the excellent dividing engine of our laboratory, which has been used for measurements of grating spectra, and which is provided with a recording attachment from designs of Kayser;[1] R and D are expressed in units of the revolution of the screw $= \frac{1}{3}$ mm. The errors of this screw, which was made according to Rowland's method, are exceedingly small and do not come into consideration in the small range covered by the measures before us. In order to furnish the reader with an idea of the magnitude of the errors in the measurements of D and D', I give the following series of observations:

Cd, λ = 5085.82	*Cd*, λ = 6438.47	*Fe*, λ = 4494.57
D = 10.500	10.490	12.435
500	495	440
515	500	425
495	482	435
480	480	442
488	495	425
Mean = 10.496	10.490	12.433

The probable errors accordingly are:

For λ	
5085.82	±3.2
6438.47	±3.2
4494.57	±2.0

expressed in units of the last place.

If the thousandth of an Ångström unit is to be accurate, the errors must not be much larger than this.

All the values obtained, referred to the green cadmium line λ 5085.824, are collected in the following tables. Each line was measured twice on different plates at different times and at different temperatures. On each photograph the three innermost rings were employed so that for each wave-length six measures are available.

[1] *Handbuch der Spektroskopie*, 1, § 567.

COMPUTATION OF THE DISPLACEMENT OF PHASE ϵ FOR THE RED *Cd* LINE λ 6438.4722

(*A*) SEPARATION OF THE SILVERED PLATES $e = 4.75$ mm; $R = 423.75$

	1	2	3	New Plate 4	5	6
Mean[1] from.	6.780	11.090	14.142	9.576	13.012	15.726
and.....	.730	.080	.138	.560	.000	.712
D.....	6.755	11.085	14.140	9.568	13.006	15.720
D'.....	10.650	14.500	17.560	7.877	12.613	16.082
$\frac{D^2}{8R^2}$.....	0.0000318	855	1392	637	1178	1720
$\frac{D'^2}{8R^2}$.....	0.0000718	1464	2146	432	1107	1800
Diff. + 1.....	0.9999529	0.9999391	0.9999246	1.0000205	1.0000071	0.9999920
P.....	18684	18683	18682	18684	18683	18682
P' by (5)....	14758.014	14757.002	14757.02	14759.013	14758.02	14757.014
$\therefore \epsilon$.....	+0.014	+0.02	+0.02	+0.013	+0.02	+0.014

The mean of these six values of the change in phase is $\epsilon = 0.017$

[1] Mean value on account of change of temperature.

(*B*) SEPARATION OF THE SILVERED PLATES $e = 3.19$ mm; $R = 426.75$

	1	2	3	New Plate 4	5	6
Mean of...	10.485	15.057	18.532	10.510	15.080	18.550
and.....	.465	.023	.500	.496	.052	.545
D.....	10.475	15.040	18.516	10.507	15.066	18.552
D'.....	11.490	16.725	20.690	10.490	16.040	20.120
$\frac{D^2}{8R^2}$.....	0.0000753	1553	2353	757	1558	2362
$\frac{D'^2}{8R^2}$.....	0.0000906	1920	2938	755	1766	2778
Diff. + 1....	0.9999847	0.9999633	0.9999415	0.0000002	0.9999792	0.9999584
P.....	12532	12531	12530	12528	12527	12526
P'.....	9899 + 0.022	9898.018	9897.015	9896.015	9895.017	9894.02
ϵ.....	+0.022	+0.018	+0.015	+0.015	+0.017	+0.02

The mean value of the change of phase $\epsilon = 0.018$

From the two mean values $\epsilon = 0.017$ and 0.018 for the different distances we may therefore construct the upper branch of the curve in Fig. 5. The lower part of the curve, as explained below, is drawn from the values of Perot and Fabry.

I now give the values from a number of iron lines which I selected at certain distances in the iron spectrum, choosing such lines as were recently determined by C. Fabry and H. Buisson.[1] The values of P' were always corrected by the curve of Fig. 5.

(*C*) SEPARATION OF THE PLATES $e=4.75$ [illegible]

Fe λ 4282.411 (Fabry and Buisson). $R=423.75$ [illegible]

				New Plate		
	7.522 .505	11.593 .580	14.564 .542	6.110 .100	10.704 .700	13.860 .830
D	7.513	11.586	14.553	6.105	10.702	13.845
D'	8.644	11.832	14.340	13.613	10.956	15.805
$\frac{D^2}{8R^2}$	0.0000393	934	1474	260	797	1334
$\frac{D'^2}{8R^2}$	0.0000520	974	1431	1290	835	1739
Diff.+1	0.9999873	0.9999960	1.0000043	0.9998970	0.9999962	0.9999595
P	18684	18683	18682	18684	18683	18682
P'	22188.975	87.975	86.975	86.975	87.975	85.975
λ'	4282.4127	.4127	.4117	.4117	.4127	.4136

Mean, 4282.4125

This differs from Fabry and Buisson by +0.001 Å.

Fe λ 4375.939 (Fabry and Buisson). $R=423.75$

	10.308 .252	13.558 .508	16.157 .100	6.070 .055	10.705 .676	13.825 .830
D	10.280	13.533	16.128	6.062	10.690	13.828
D'	9.753	12.709	15.080	10.150	13.052	15.370
$\frac{D^2}{8R^2}$	0.0000735	1275	1811	256	795	1331
$\frac{D'^2}{8R^2}$	0.0000662	1124	1583	717	1186	1644
Diff.+1	1.0000073	1.0000151	1.0000228	0.9999539	0.9999609	0.9999687
P	18683	18682	18681	18684	18683	18682
P'	21713.98	12.98	11.98	13.98	12.98	11.98
λ'	4375.9424	.9444	.9454	.9444	.9414	.9434

Mean, 4375.9435

This differs from Fabry and Buisson by +0.004 Å.

[1] *Comptes Rendus*, **143**, July 1906.

Fe λ 4494.576 (Fabry and Buisson). $R=423.75$

	9.241 .227	12.740 .730	15.505 .468	10.355 .312	13.605 .570	16.183 .152
D	9.234	12.735	15.486	10.333	13.587	16.168
D'	8.018	11.489	14.161	9.268	12.407	14.920
$\frac{D^2}{8R^2}$	0.0000593	1129	1669	743	1285	1820
$\frac{D'^2}{8R^2}$	0.0000447	919	1396	598	1071	1550
Diff. + 1	1.0000146	1.0000210	1.0000273	1.0000145	1.0000214	1.0000270
P	18683	18682	18681	18683	18682	18681
P'	21140.98	39.98	38.98	40.98	39.98	38.98
λ'	4494.5802	.5812	.5812	.5812	.5833	.5802

Mean, $\lambda=4494.5812$

This differs from Fabry and Buisson by +0.005Å.

Fe λ 4859.759 (Fabry and Buisson). $R=423.75$

	8.730 .757	12.447 .428	15.221 .245	7.560 .542	11.6 .0	14.627 .615
D	8.744	12.437	15.233	7.551	11.626	14.621
D'	9.322	12.702	15.328	8.168	11.898	14.737
$\frac{D^2}{8R^2}$	0.0000532	1076	1615	396	941	1488
$\frac{D'^2}{8R^2}$	0.0000605	1123	1635	464	985	1512
Diff. + 1	0.9999927	0.9999954	0.9999980	0.9999932	0.9999956	0.9999976
P	18684	18683	18682	18684	18683	18682
P'	19552.99	51.99	50.99	52.99	51.99	50.99
λ'	4859.7606	.7606	.7617	.7629	.7617	.7606

Mean, $\lambda=4859.7613$

This differs from Fabry and Buisson by +0.002Å.

The accompanying data have been given in the first instance to illustrate the adequacy of the method. Some of the measurements are entirely satisfactory in regard to their agreement, while for others deviations as large as 0.006 units occur. This is obviously due to the intensity or structure of the line under investigation, the diameter of whose ring is determined with less exactness. To get an idea of how easily

Fe λ 5232.960 (Fabry and Buisson). $R=423.75$

	7.487 .481	11.600 .624	14.598 .582	5.973 .934	10.637 .631	13.782 .776
D	7.483	11.612	14.590	5.954	10.634	13.779
D'	10.410	13.726	16.405	9.300	12.873	15.674
$\frac{D^2}{8R^2}$	0.0000390	938	1482	247	787	1321
$\frac{D'^2}{8R^2}$	0.0000754	1311	1873	602	1153	1710
Diff. +1	0.9999842	0.9999627	0.9999609	0.9999645	0.9999634	0.9999611
P	18684	18683	18682	18684	18683	18682
P'	18158.0	57.0	56	58	57	56
λ'	5232.9602	.9626	.9614	.9650	.9662	.9626

Mean = 5232.9630

This differs from Fabry and Buisson by +0.003Å.

errors can occur, it should be considered that a deviation of a thousandth of an Ångström unit corresponds to an error of 0.005 (=0.0016 mm) in the measurement of the diameter of the ring, D being taken as equal to 13 in the average, and D' assumed to be correct. The conditions are indeed somewhat more favorable to the computation for the smaller rings, but the error of reading on account of the lack of sharpness of the rings doubtless increases in a similar degree, so that nothing is gained thereby. When we recall that the results were obtained under the most varied conditions, that large and small rings were taken, that comparisons were made with rings of higher and of lower order, that different photographs were used, etc., we obtain a certain confidence as to the correctness of the figures. It is nevertheless surprising that the values found here are all larger than those of Fabry and Buisson; this leads to the suspicion that the deviations come from some systematic cause, and it will be the next duty to seek this where the weak side in the method lies. There is no doubt but it will be found to lie in the displacement of phase. Thus Lord Rayleigh[1] finds a decidedly larger difference between the green and red cadmium lines than I have and than was established by Perot and Fabry.[2]

[1] *Loc. cit.*, p. 700.

[2] *Astrophysical Journal*, 15, 95, 1902.

The results are:

Perot and Fabry	$\epsilon = 0.013$
Eversheim	$= 0.018$
Lord Rayleigh	$= 0.050$

It is striking that Rayleigh finds the same displacements of phase for the blue as for the green cadmium line,[1] whence we might conclude that for the green line the limit of penetration is already reached. There is, however, evidence in favor of the view that the blue line possesses peculiarities which must disturb the measurements. I have in fact been obliged to conclude that there are some phenomena which at present are unknown to me, and I was unable to determine the correction ϵ in the same way as for the red line. However, I reserve it for a later test. I must content myself with completing the lower branch of the curve in Fig. 5 from the values of Perot and Fabry.

It is my purpose to carry out the work of establishing normals as rapidly as possible, and it is my intention to employ the method adopted by Perot and Fabry. By this method the light is dispersed after its passage through the silvered air-stratum, and we then get a spectrum the lines of which are composed of a number of small arcs which correspond to the interference rings. In this way with one process we obtain a large number of lines on the photographic plate which can then be measured just as are the single rings.

[1] He assumed for the red line $\epsilon = 0$.

ON THE CONSTANCY OF WAVE-LENGTH OF SPECTRAL LINES

By H. KAYSER[1]

An extensive literature has already grown up on the question whether the wave-lengths of spectral lines are invariable or whether they depend on the mode of production of the spectrum, whether the density of the vapor has any effect, etc. I have just received a paper by Exner and Haschek,[2] who have been the principal representatives of the assumption of variability of wave-lengths, in which the authors attempt to add new evidence for their view. The importance of this question in terrestrial and astronomical spectroscopy leads me to make some remarks on the subject. The gentlemen wish to establish a case on three measurements of the spectrum of lanthanum made by different students in my laboratory, in which as a matter of course differences of wave-length occur amounting to several hundredths of an Ångström unit. These differences Exner and Haschek seek to interpret as proof of the variability of the wave-lengths.

This explanation is in my opinion incorrect. A principal reason for the differences appears to me rather, to lie in the errors of the standards from which the wave-lengths were determined. I have previously referred to this effect, stating,[3] "I am convinced that the much larger differences (than several thousandths of an Å) which different observers obtain for the same line are due to the fact that they depart from different standards, which do not agree with each other."

It is true that the three gentlemen all took their standards from my table of the iron spectrum or from Rowland's tables for the Fraunhofer lines; but they took different lines as standards, whereby, in view of the inaccuracy of the standards, errors of 0.02 Å can easily arise. This very fact that with the same standards an accuracy of a few thousandths Å is attained, while with different standards only as many hundredths of a unit, led the International Solar Union to

[1] Translation from the *Zeitschrift für wissenschaftliche Photographie*, **5**, 304–308, August 1907.

[2] *Sitzungsberichte der Wiener Akademie*, **116**, IIa, 323–341, 1907.

[3] *Zeitschrift für wissenschaftliche Photographie*, **2**, 50, 1904.

place upon its programme the more precise determination of the standards as one of the most pressing problems.

A comparison of the series of measures shows clearly that the insufficiency of the standards is principally to blame. My iron standards were produced by the adjustment of many errors in Rowland's values, and they therefore agree very much better among themselves than Rowland's solar standards. Therefore in the region from λ 2200 to λ 4500, in which my standards were employed, the differences must be smaller than for greater wave-lengths. This is confirmed as follows: In the first region Kellner measured 151 lines, in the second 88 lines. Differences between him and Wolff in amounts of 0.06 and 0.05 occur, in all, four times, and these only in the second region. The difference 0.04 occurs seven times in the second region, but only three times in the first, in spite of the twofold number of lines in the first region. The difference of 0.03 occurs only half as often in the second region; and only the small differences of 0.01 and 0.02 are uniformly distributed over the two regions.

In addition to this principal cause of the differences, errors of measurement also surely occur for some of the lines. Practice is necessary for correct measurement, particularly for lines that are not quite sharp, and the measures referred to here were by beginners. Individual errors will always arise, even if I also check the measures and convince myself by tests of their general correctness.

While I differ from Messrs. Exner and Haschek in the explanation of these differences of measurement, I cannot see in the numerical values any evidence whatever for a displacement toward the red. On the contrary it is easy to see again here that errors of the casually selected standards sometimes make the measurement larger and sometimes smaller. I compare here only the longer waves according to Wolff and Kellner: from λ 447 to λ 467 the values of the former are all larger, from λ 469 to λ 510 smaller, from λ 510 to λ 516 larger, from λ 516 to λ 526 again smaller, from λ 526 to λ 538 larger, from λ 545 to λ 551 smaller, and then about equal to the end of the measurements. We see from this that the differences always cover larger portions of the spectrum, as would be the case in inaccurate standards. The correction curve of the one measurement with respect to the other would be a sort of sine curve, and no conclusions of any sort

are justified from these figures as to a displacement toward the red or toward the violet.

Further proofs of the constancy of wave-lengths have happily been brought out very recently which could not at the time have been known to Messrs. Exner and Haschek. The condition set by the Solar Union that a new system of standards should be produced, which should all be correct within a few thousandths of a unit, obviously could be carried out only if the wave-lengths are invariable. Therefore this fact had to be tested first. At the meeting of the Union in Paris in May, Professor Ames was able to communicate the fact that Dr. Pfund had made experiments in his laboratory with the interferometer which proved that the wave-lengths are precisely the same, regardless of whether the spectrum was produced in the spark or in the arc, at atmospheric pressure or in a vacuum, of pure metals or of an alloy or salts. This was true without exception for all the elements investigated. Professor Fabry declared that his experiments had yielded precisely the same result. Inasmuch as the most precise method which we have was employed here, we must regard these experiments as decisive, and consider that the question of the constancy of the wave-lengths is finally settled.

In the second part of their paper Messrs. Exner and Haschek discuss another phenomenon which to me does not seem quite in place there, or at least alters their position in comparison with their former view. I am probably not wrong in assuming that the gentlemen herewith abandon a part of their former view. They assumed formerly that the lines were displaced continuously and accordingly to law with the density of the vapor; indeed a quantitative analysis was to be based on this displacement. Now they say that many lines are formed of numerous components and under various forms of excitation of the spectrum, one or the other of these components may become stronger; the center of gravity of the combination might then be displaced in the grating spectrum, in which the line is not resolved. It is quite clear, and was long ago pointed out, that this case is possible and that in this sense variable lines can exist; but it is equally obvious that this could never occasion a continuous displacement proportional to the density of the vapor. Furthermore, it would be just as probable that the component line toward the violet should be

the stronger as the one toward the red. It may have been merely a matter of chance if Exner and Haschek observed components to be stronger only on the red side when working with an inadequate echelon.

It is further to be remarked that the number of lines resolved by the echelon into components is vanishingly small in comparison to the number of simple lines, if we may judge from the little we thus far know on this subject—hardly more than one investigation, by Nutting, can be mentioned.

It follows from this that that sort of variability of wave-length may indeed theoretically occur, but only for a small number of lines; and that this variability has nothing to do with the continuous displacement previously asserted to exist. I also know of no case where an actual displacement could be proven from this cause. Only in the case of the cadmium line λ 5086, which Exner and Haschek also mention, has a variability of the components been safely established, by the observations of Hamy and Fabry. In spite of this, this line is an excellent standard of invariable wave-length.

But if we assume that actual and marked variations of components frequently occur, then the result is simply what Eder and Valenta and I have asserted from the beginning: the wave-length remains constant *if correctly measured;* that is, if the position of the principal component, of the maximum, is determined; and this is equally true for composite lines and for lines unsymmetrically widened.

I am glad to see from Exner and Haschek's paper (p. 337) that they are now of this same opinion; for, in withdrawing their assertion that actual displacements are involved, they actually recognize that only apparent displacements are in question and that the wave-length remains unchanged with a correct measurement.

They are confirmed in this view by the above-mentioned results of Ames and Fabry, and herewith no difference of opinion may be expected to continue regarding this question, which has been decisively settled by experiment.

BONN, GERMANY
July 1907

Reviews

A General Catalogue of Double Stars within 121° of the North Pole. By S. W. Burnham. Published by the Carnegie Institution of Washington, 1906. Price, $14.00.

The monumental works in any field of science are not numerous. Two have now appeared in the department of double-star astronomy: Struve's *Mensurae Micrometricae*, bearing the date 1837, and Burnham's *General Catalogue;* the title of which appears as the heading of this article. It is true that many great works on double stars have been published, but these two stand above the others in comprehensiveness and enduring qualities.

The first general catalogue of double stars was printed in 1820 by Wilhelm Struve, enumerating the 795 objects of this class then known from the north celestial pole to 20° south declination. Seven years later, after his extensive exploration of the northern heavens, resulting in a vast number of discoveries, Struve published his *Catalogus Novus Stellarum Duplicium et Multiplicium*, listing 3,112 objects from the north pole to 15° south declination. This became Struve's working programme and after he had measured all the stars in it which he deemed worthy of retention, he printed his measures in *Mensurae Micrometricae*, a work which summed up to the time of its publication all that was best in the observational data respecting the stars to which it relates. Moreover, it was a finished work, which at once became an example and guide, giving direction and character to double-star investigations, which have continued in force to the present.

However thoroughly we may sum up the accomplishments in a large department of science to a given epoch, the subject does not rest, but new results continue to appear, making formidable additions to those already collected. Thus, while Struve was making the observations which are published in *Mensurae Micrometricae*, Sir John Herschel was exploring the skies of both hemispheres, making many discoveries, which he announced from time to time in various catalogues, which eventually included more objects than had been registered by Struve. A few years later, in 1843, Otto Struve's *Pulkowa Catalogue* appeared, and after that, although observations continued to be made in great numbers, the discovery of new

pairs rested for a space of nearly thirty years. It was revived by Professor Burnham, at Chicago, experimenting at first with a little instrument just good enough to make something better desirable. Fortunately a better instrument was soon acquired, and then he startled the astronomical world with the revelation that the explorations of the Herschels and the Struves were not complete, but that virgin fields remained even to the possessor of a six-inch refractor.

Burnham had not used this instrument long before he felt the need of a complete and reliable list of the double stars then known, and to supply this want he began to collect the material which has grown into the volumes before us. In the introduction he tells us how it came about. The small refractor, from the beginning, was used almost entirely for the observation of double stars. Objects were constantly found with it which could not be identified in any of the books at hand for reference. At that time there were few books in Chicago relating to double stars, and no complete catalogue existed of those then known. To meet his needs he began to form a manuscript catalogue from the books that were accessible to him. Observatories were visited and their libraries consulted; books were borrowed and their contents noted; books and memoirs were purchased, making the beginning of a library pertaining to this subject that has become practically complete. The manuscript catalogue formed at the outset at so great a cost of time and labor has been kept continuously posted to date, by the addition of all new stars and new measures from current publications. In order to make room for new material a second manuscript edition eventually became necessary, and still later a third, which finally passed into the hands of the printer, and now appears in finished form.

This work is in two parts. Part I, which is complete in itself, contains the *Catalogue* proper, enumerating 13,665 double and multiple stars from the north celestial pole to 31° south declination. This fills 274 large pages, including an appendix of 18 pages giving the double stars announced from the Lick Observatory while this work was passing through the press. The introduction, devoted mainly to a brief description of the volume; and the indices and precession tables, fill 55 pages additional. Part II contains the *Notes to the Catalogue*. These fill 838 closely printed quarto pages.

Part I is in tabular form, with eleven columns to the page. The stars are arranged in the order of their right ascensions and are numbered consecutively. These reference numbers are given in the first column. The second column contains the name of the star according to its usual desig-

nation in this department of astronomy; and the third, its name or constellation letter or number, or failing these, its number in some standard catalogue of stars. The fourth and fifth columns contain the right ascension and declination for the epoch 1880, an epoch which was selected for this work before any of the *A. G.* catalogues were printed. The data given in the remaining columns are the position angle, distance, and epoch of the earliest reliable measures; the magnitudes of the components; indication of the observers and the number of nights on which measures were made; and brief notes, usually relating to notation and colors of components and such references as may be compressed into a short line. Many double stars have to the present but a single set of measures. This is especially so for many of the pairs recently discovered. Often all data of interest respecting such a star may be given in a single line, and such pairs are not always mentioned in the notes in Part II.

The notes in Part II are given in the order of the stars in Part I; that is, in the order of their right ascensions, which, for convenience of reference, are here placed in the margins of the pages. Each note is preceded by the reference and double-star numbers of the pair, and also, when it has them, by the star's name and synonym. The notes are reduced to a very compact form, giving in a few words an outline of the star's history, selected measures for various epochs, conclusions respecting relative and proper motions, and, what is most valuable, complete references to all published observations. These are the data most useful for general purposes and they are presented in a form above criticism. Diagrams often accompany the notes to the binaries and proper motion stars, especially when the change in the relative situations of the components has been considerable. These diagrams, by graphically picturing the motion, render it the less necessary to quote long lists of measures, and the author has elected to give these rather sparingly. On some accounts it might have been better to have quoted larger numbers of observations, enough, when they are available, to enable one to form an independent opinion as to the character of the motion of any given pair. Now one has generally to rely upon the author's conclusion, or turn to the original sources, which may not be more accessible to a given investigator than they were to the author when he began this work thirty-seven years ago.

Except in the case of *β Delphini*, the only element of the orbits of the binaries quoted is the periodic time. It is to be regretted that the other elements are not also given, for they too are necessary to the making of exact comparisons between observed and computed places.

This work is not a mere compilation, as might, perhaps, be inferred

from what precedes. On the contrary, in its presentation of new and important facts, it is a great contribution to knowledge. Its highest merit resides in its reliability, which could not have been secured by consulting publications alone. For many years Professor Burnham has been a most industrious observer, having had at his command at various times, some of the largest and best telescopes of the world, among them the large refractors of the Washburn, Dearborn, Lick, and Yerkes observatories. With each of these instruments he has set at rest many questions which could only be answered by an appeal to the sky, and then oftentimes only by the faithful following of particular stars through many years.

Professor Burnham has endeavored to bring the histories of all the double stars in this work as nearly up to date as possible, and this has necessitated the re-observation of the neglected pairs. For some years he has devoted himself unremittingly to this task, and in these volumes he has given us for the first time the mean results of several thousand observations, made to fill this special need. When we look over the record and remember that these new measures were obtained by observing on two nights only per week, we wonder at his accomplishment.

Among the features which increase the value of this as a reference work, are the tables in Part I, following the introduction. Here, in compact form, the double stars discovered by modern observers are conveniently indexed, enabling any pair to be readily found. Tables follow, giving a provisional grouping of those stars which have given evidence of their character by means of their motions. Precession tables are also provided, which will be very useful in comparing the places of the stars at different epochs, as may be necessary in verifying identifications.

So meager are the data to the present, even when collected as given in this work, that it is not possible to make more than a beginning in the separation of the double stars into the different classes to which they belong, putting the binaries in one group, those whose components have a common proper motion in another, and so on. According to Professor Burnham's tables, just mentioned, the number of objects in each class is as follows:

Binaries with computed orbits	88
Binaries without computed orbits	94
Stars probably binary	112
Stars of the type of *61 Cygni*	38
Stars with common proper motion	579
Rectilinear motion	387
Total	1,298

If this classification were made by another person, the numbers would doubtless be altered slightly, by the transfer of certain stars from one group to another. For example, I should place *61 Cygni* among the stars probably or certainly binary. But the general result would remain the same. The table is an impressive commentary on the slowness with which the substantial facts concerning the apparent movements of the stars are obtained. Out of the 13,665 double stars enumerated in this great catalogue, and of which roughly 6,000 have been known for more than sixty years, only 88 are classed as binaries with orbits computed, and for more than half of these the elements are so uncertain that little reliance may be placed in them. It is even doubtful whether some of them are binaries. Thus, an orbit has been computed for λ *Cygni*, but so far as may be judged from the observations, this pair is as likely to prove a case of rectilinear motion as a binary combination.

Further, from the table above it will be seen that less than 10 per cent. of the stars which have been catalogued as double have moved sufficiently since their first observations were obtained to enable us to form an opinion respecting the character of their motions, and exclusive of the pairs whose components have a common proper motion, only $2\frac{1}{2}$ per cent. are as yet known to be binaries. That the number of proven binaries is so small, doubtless results from the too generous inclusion of wide pairs by the earlier observers, and to the fact that nearly one-fourth of all the stars listed are recent discoveries, which have been measured at one or two epochs only. The discoveries of the past decade include a large majority of the close double stars, and when these are measured anew, at intervals sufficiently separated, the percentage of established binaries will doubtless be materially increased. Here, as in other departments of this subject, observation is the way of progress, and in this Professor Burnham's work will be a powerful aid, supplying what has hitherto been wanting, the means of judiciously selecting an observing programme.

The need of a general catalogue of the known double stars, which Professor Burnham experienced thirty-seven years ago, has been felt by every worker in this field who has passed beyond the boundaries of the subject and attempted to add material of value to this department of knowledge. The exigencies of the situation have imposed upon some of us the necessity of forming manuscript catalogues of the known double stars to facilitate our own investigations. Thus, when the systematic survey of the stars to the 9.1 magnitude was begun in 1899 at the Lick Observatory, I found it necessary to search through hundreds of publications, as Professor Burnham had done, and construct manuscript lists of

the double stars contained in them. In doing this my labor was less arduous than Professor Burnham's, for I did not attempt to collect all the published observations of the stars, and moreover, I had at my command what he did not have at the beginning, a large astronomical library, with many references in this particular department. Nevertheless, the labor involved in this, and the making of nearly ten thousand observations, and in the telescopic examination of many thousands of stars besides, was sufficient to give me an appreciation of the magnitude of his accomplishment. The production of his *General Catalogue*, from whatever standpoint it may be viewed, must be regarded as one of the great, single-handed achievements in astronomy.

W. J. HUSSEY

ANN ARBOR, MICH.
September 1907

Stereoskopbilder vom Sternhimmel. 1. Serie. Von MAX WOLF. Leipzig: J. A. Barth, 1906. 5 Marks.

This little portfolio of photographic prints embraces a most interesting series of twelve celestial "stereograms" arranged in the following order: 1, variable star *R Coronae;* 2, *Saturn* and two of his moons; 3, asteroid trail—*Svea*, No. 329; 4, meteor trail; 5, 6, 7, three views of Comet *b* 1902; 8, star showing large proper motion; 9, *Andromeda* nebula; 10, *Orion* nebula; 11, lunar *Apennines;* 12, region around the lunar crater *Albategnius*. Accompanying each print is a double page of descriptive matter.

Examination of the views shows that not all are true stereograms, as it is obviously impossible to obtain the effect of relief upon such objects as the *Orion* or *Andromeda* nebulae; this is remarked upon, however, in the letter-press.

If any adverse criticism could be made of this generally excellent series, it might be that those of the moon (from the negatives of Loewy and Puiseux) are the least pleasing from an optical standpoint, while it might be objected that Nos. 5, 6, 7, 11, and 12, have suffered such enlargement as to render the silver grain particles objectionably apparent. In Nos. 9 and 10 an improvement could have been effected by judicious chemical reduction of the negatives, whose "contrast" is fatal to the portrayal of detail.

Considered as a whole, the series is good, and should meet with prompt appreciation and a ready sale.

R. J. W.

A GENERAL INDEX TO THE ASTROPHYSICAL JOURNAL

The preparation of an index to the first twenty-five volumes of this Journal, covering the twelve and one-half years from January 1895, to June 1907, is now under consideration. Such an index would doubtless prove of great convenience to the workers in astrophysics and to libraries. The possibility of its publication will depend upon the number of advance orders received. If 200 subscriptions are obtained, the index can probably be issued; if 300 advance orders should be given, the work will certainly be undertaken, with the expectation of its publication in the winter of 1907,–8 and the price will probably be $1.50.

All subscribers and librarians who would purchase such an index, if issued, are therefore requested to notify the publishers at once by postcard of the number of copies for which they will subscribe.

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In the department of *Minor Contributions and Notes* shorter articles will generally be placed and subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Accuracy in the proof is gained by having manuscripts type-written, provided the author carefully examines the sheets and eliminates any errors introduced by the stenographer. It is suggested that the author should retain a carbon or tissue copy of the manuscript, as it is generally necessary to keep the original manuscript at the editorial office until the article is printed.

All drawings should be carefully made with India ink on stiff paper, usually each on a separate sheet, on about double the scale of the engraving desired. Lettering of diagrams will be done in type around the margins of the cut where feasible. Otherwise printed letters should be put in lightly with pencil, to be later impressed with type at the editorial office, or should be pasted on the drawing where required.

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Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

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